SCIENTIFIC AMERICAN SUPPLEMENT

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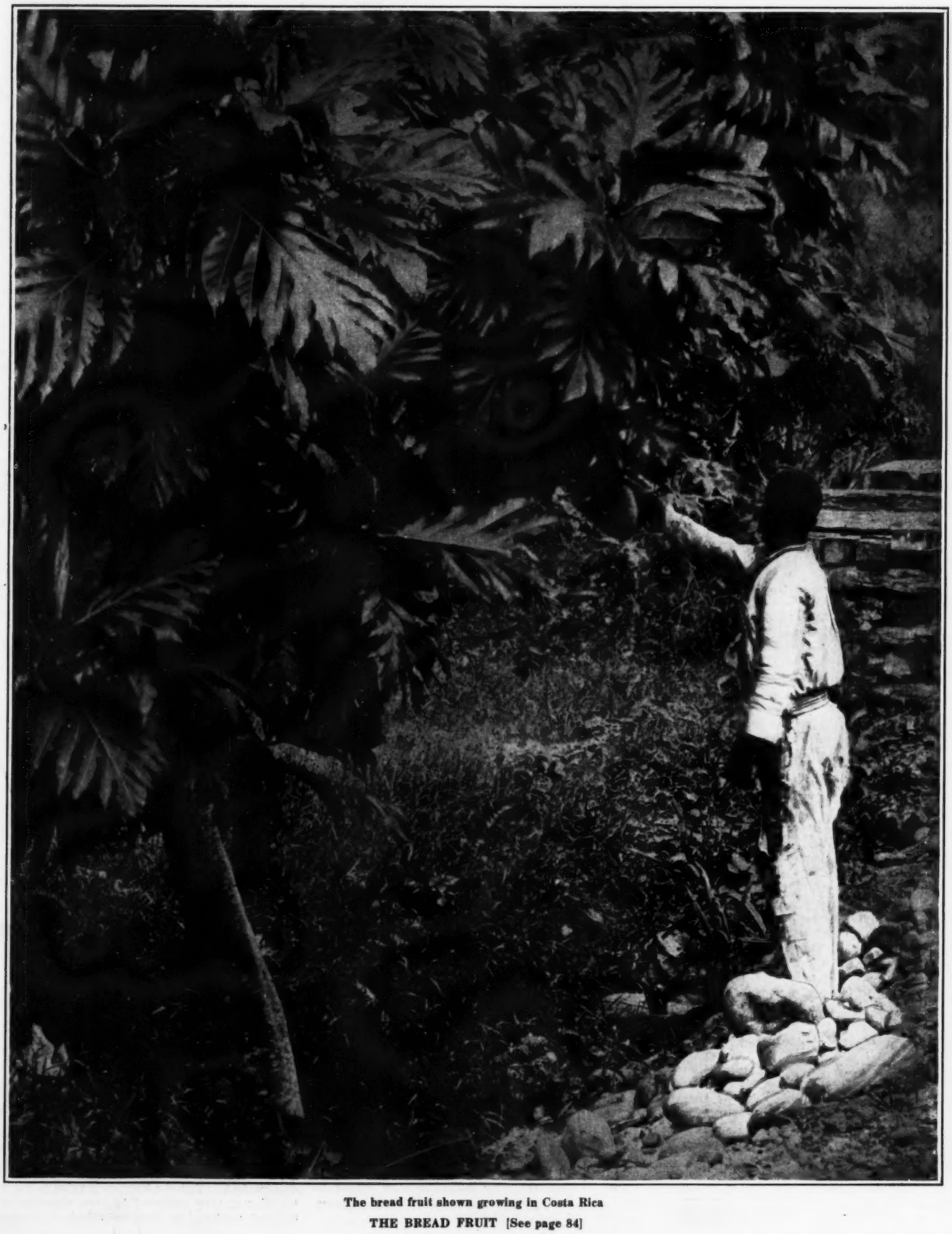
VOLUME LXXXV]
NUMBER 2197]

★ NEW YORK, FEBRUARY 9, 1918 ★

[10 CENTS A COPY
$5.00 A YEAR



The bread fruit shown growing in Costa Rica

THE BREAD FRUIT [See page 84]

Direct Selection in Pure Lines*

Recent Studies in Plant Improvement

By Professor Hugo De Vries

Louis Lévêque of Vilmorin was the first to introduce the method of selection in the cultivation of agricultural plants. Applied to domestic animals, this practice had already given important results, and he looked for ones of equal value from its application to the beet plant. He soon perceived that to obtain the most rapid improvement it was necessary to practice selection not merely among the mother plants, but also among separate families, each descended from a single individual. By comparing the progeny of parents isolated according to this principle, it becomes possible to choose the family best fitted to improve the race. The old race itself was thus simply replaced by the new one.

The principle consisting in cultivating geneological families, and in judging the value of the plants not alone by their own visible characters, but, above all, by the degree of superiority of their progeny, did not obtain at this period the favor which it merited, and during the second half of the last century the almost universal rule in agriculture continued to be selection among mixed sowings. This method has been amply described by Wilhelm Rimpau, who applied it with great success to the improvement of his rye. According to his description we may now conclude that the variety which he made use of for his point of departure was in reality a mixture of more or less numerous types, some of which were superior, and others inferior, to the average. By means of selection he succeeded, in the course of from 10 to 20 years, in isolating one of the richest races of this primitive mixture, without, however, comprehending the process as we at present comprehend it. Had he understood it, he would have sown separately the seeds of the plants of his initial choice, and by the comparison of their progeny he would have achieved exactly the same degree of improvement in only a few years. The isolation of the mother plants, and the determination of their industrial value by that of the average of their progeny, have been recently introduced into agriculture by Hjalmar Nilsson, the director of the Swedish experimental station at Svalöf, and by W. M. Hays, of the Minnesota experimental station. Their methods and their results have been amply discussed in my book on the breeding of plants, and presented to the agricultural public of nearly all civilized nations.

In my studies on the experimental production of mutations in the genus *Oenothera*, I introduced this principle in 1895, combining it with artificial autofecundation. At this period I had already cultivated three generations of *Oenothera Lamarckiana*, cultivating them in little groups, entirely separated from other forms of the same genus; but the flowering took place in the open, and fecundation was accomplished through the agency of bees and moths. The object of these preliminary cultures was to assure myself that the phenomena of mutation, manifested in the open field, could be obtained quite as well in the gardens. Consequently the conditions of the culture were made as little different as possible from those in a state of nature. The direct observation of the phenomenon of mutation was a novelty at that period, and it was necessary to take all possible precautions to be perfectly sure that the sports of the garden were of the same nature as those of plants growing wild.

As soon as this essential point was proved beyond a peradventure, I had recourse to artificial fecundation, and to the preservation of the seeds of each mother plant separately. This method led to the culture of genealogic families, each having for its origin an individual plant in which I had myself secured auto-fecudation. It is easy to obtain auto-fecundation in the flowers of the *Oenothera* of Lamarck, and with an abundant yield of seeds. To do this one surrounds the flowers with paper bags large enough to protect the principal portion of the raceme, after cutting off its top, against the visits of insects. The seeds of each mother plant were always preserved and planted separately, and this rule has been followed invariably every year since my cultures began. For some time, however, the large bags have been replaced by smaller ones, each of which encloses but a single flower.

The same method of artificial auto-fecundation carried on through each successive generation, and of the separate preservation of the seeds of each plant, has been applied to the mutants born of my principal genealogic line. The most interesting example has been that of the *Oenothera mut. gigas*, which appeared for the first time, in a single specimen, in 1895. This plant was biannual and in the following year was carefully auto-fecundated by me. It provided me with a pure genealogic line during four successive generations, and the seeds of each generation have served me for a number of years for different crossings and other experiments. In the same manner other primitive mutants have been derived from other pure families, and in some cases I have had several lines belonging to the same type, but having for their point of departure different individual mutants.

The principle of experimental genealogic families has demonstrated its high scientific value in my cultures of various other plants capable of auto-fecundation, and has met with general approbation. Naturally, it applies only to plants, and among these to those species only which can be auto-fecundated with sufficient success. In the case of all other species it is necessary to derive the family from two initial plants, and at each generation at least two individuals must be utilized to continue the race. Such is the case, for example, with the sunflower, the red clover, the diœcious plants, and with animals. In those cases in which the choice of the initial individuals permitted the isolation of two individuals with the same exterior and hereditary characters, the results of the cultures have been the same as in the families springing from a single individual and continued in each generation by the seeds of a single specimen.

To this latter type of genealogic family Mr. Johannsen has given the very appropriate name of "pure lines," and the extended application which this eminent author has made of this principle has contributed largely to its general acceptation in studies of heredity.

Pure lines are the genealogies of auto-fecundated families derived from a single initial individual, and maintained in each generation, without interruption, by the seeds of a single specimen, which has itself also been auto-fecundated.

Naturally there are numerous types of pure lines and some distinctions are necessary. The most evident is that which must be made between "lines really pure" and "lines half pure," or demi-pure. The first are derived from a mutant and comprise all its progeny, at least in so far as it results from artificial autofecundations. Their commencement is that of a new race, and the pure line may embrace all the new variety, or all the elementary race, in such measure as one may desire. The commencement, here, is a natural phenomenon.

The "demi-pure lines," on the contrary, are derived from an individual arbitrarily chosen from some existing race. This race may be what one might call a natural pure line, and may consist solely of individuals having the same hereditary qualities, as is the case with elementary species, and with many varieties; or, on the other hand, the initial race may be a mixture of more or less numerous distinct types, and this is notably the case with the majority of agricultural plants. In the third place we may select the initiators of a pure line among a population of individuals whose affinities have not been studied, and among which, consequently, there may be hidden distinct hereditary types. In the two latter cases the initial choice of a single plant will evidently isolate one of the constituent types of the population, and it is clear that the qualities of the pure line derived from this individual will depend principally upon this choice.

When we obtain pure lines from elementary species all the lines derived from different individuals of the same species must be essentially the same, provided there are neither crossings nor mutations. Let us take once more the *Oenothera* of Lamarck as an example. This form, introduced from America into Europe about a century ago, is a pure elementary species. I have derived from it a number of experimental pure lines, taking the initial plants either from my first cultures, as in 1895, or directly from the open fields, as in 1905, and on other occasions. Each line presented the same qualities, and particularly the same constancy of the principal stock, the same mutations with respect to this, and the same phenomena of doubling on being crossed with certain other forms.

In the wild state and in ordinary gardens the *Oenothera* of Lamarck is ordinarily pure, though in certain places it is found mixed with its mutants, as on the coast of Liverpool, or with other species of the same genus and with their hybrids, as in certain points on the dunes of Holland and of France. Cases of this kind give the impression of polymorphism, but everywhere where other species are lacking, and where the mutants do not succeed, or succeed only exceptionally, the purity of the line is very striking. For these reasons we may call the original elementary species a natural pure line.

In other cases the pure lines may be very different from the described type. We may cite the *Oenothera biennis*, an old and very constant species, in which, however, the pollen is the vehicle of hereditary qualities other than those conveyed by the ovules. Other lines may present regular doublings in each generation, as is the case with the *Oenothera Lamarckiana mut. scintillans*, whose truly pure lines, derived directly from a mutant, produce each year a considerable number of specimens returning to the type of the original species.

In pure lines the floating variability may be either slight or considerable. In the cereals, beans, and other agricultural plants, it is commonly very slight, and scarcely establishes essential differences between different individuals. But in other cases the amplitude of variation is very striking. Let us take for example the well-known polycephalous from of the ordinary poppy, *Papaver somniferum polycephalum*.

By the term polycephaly we understand the transformation of a greater or smaller number of stamens into supernumerary carpels. These may consist each of a single stamen, or else of groups of stamens combined in a more voluminous organ. These lateral carpels may have one or several stigmata, and their ovary may contain one or more chambers, in which the ovules are often quite numerous; but it is rare to find fertile seeds in them. These rare seeds do, in fact, yield plants reproducing the anomaly, but the same thing is true of the great central ovary.

By cultivating the seeds of a single auto-fecundated head under different conditions one may easily perceive that the degree of development of the crown of lateral carpels depends in great measure on the external conditions of the culture. The plants which are favored by these show full crowns with no gaps all around the central fruit. Those which are developed under less favorable conditions have crowns which present more or less numerous gaps; and among plants which have lived in still worse conditions there are almost no lateral carpels. Exposure to full sunshine, plenty of space, a fertile soil, and above all a manure rich in nitrogenous matter, *etcetera*, are the factors which increase the number of secondary carpels. The individual vigor of a plant, the direct result of these factors, may be measured by the dimensions or by the weight of the first fruit of the plant, and then reveals a close parallelism with the development of the crown of adventitious carpels.

Thus we see that the fluctuating variability depends more or less directly upon the nourishment, and this throws a special light upon the question of selection in pure lines. If we select the extremes we simply choose the best or the worse nourished individuals, and it is evident that their seeds will be in corresponding condition. Germination and the first, and most delicate, stage of growth, will be favored in the one case and hindered in the other. This sort of selection may easily occasion a deterioration in the line, but evidently this will be only so long as the external conditions of the culture remain the same.

The most interesting result of such a selection is the discovery that it is not possible to isolate from one pure line either a line having only full crowns, or, on the other hand, a line having no traces of polycephaly. The two extremes are not distinct races mingled in the principal lines, but the extremes of an extensive fluctuating variability.

These facts are corroborated by many others, and notably by the study of races having teratological features, such as fascies and torsions. In these the most striking fact is that the hereditary value is not parallel to, nor even indicated by, the ensemble of the external marks of a given individual. The seeds of

*Translated for the SCIENTIFIC AMERICAN SUPPLEMENT from *Scientia* (Rome), I-VII, 1917.

the individuals, which are themselves the most completely fasciated, or twisted, do not produce a larger number of typical individuals than do those of other specimens of the same line. We may, on the other hand, choose as a selective character this very quality of the comparative richness of the progeny in individuals of the chosen type of the race. This percentage of typical specimens among the progeny is the true measure of the hereditary value of a mother plant, and we may define it as the hereditary percentage.

There are two essentially different principles of selection, therefore, which may be distinguished by the names of direct and indirect methods, and which may be applied to lines which are pure in the strictest sense of the word as well as to the ordinary pure races. The indirect method causes selection according to the visible characters of the plants. It would choose poppies having a full crown, or the most fasciated stalks, or plants showing the greatest degree of torsion, and so forth. But, as we have seen, the plants thus characterized furnish us no guarantee that the average constitution of their progeny will be superior to that of any other specimen of the same race.

Direct selection, on the contrary, neglects the visible characters, and takes into account only the average composition of the progeny. The indices by which it is guided are the hereditary percentages of autofecundated individuals, when auto-fecundation is possible, or otherwise of specimens isolated in groups as small as possible. It chooses for the continuation of the race the individuals whose progeny presents the greatest number of specimens of the desired type. In this manner it counts on improving the average of the race, and does in fact succeed in this in a few generations.

It must be admitted that from the practical point of view the names given to these two principles seem not quite adequate. The visible marks offer themselves to immediate inspection, while the hereditary percentages can not be determined till later, long after the season of flowering, and often, as in annual and biannual plants, only after the death of the individual. But from the theoretic point of view it is clear that, in order to increase the hereditary percentage of typical individuals in a race, this percentage must itself be chosen as the criterion of selection. From this point of view the method fully merits the title of "principle of direct selection."

The matter may be presented in still another fashion. In a pure line the hereditary qualities are transmitted from one generation to another in an uninterrupted series. The characters of the individuals which constitute the line depend upon it for the most part, but not exclusively. They are influenced also by the external conditions of life, and in many races so much so as to almost completely hide the effects of heredity in a greater or smaller number of specimens.

The hereditary qualities, or the *genotype*, as Johannsen has called them, must be regarded as the primitive or primary character, the visible qualities, or the *phenotype*, being only the external expression of it. In selection the choice should be based, evidently, upon this primary character, and not upon expressions which may be either present or absent, or developed in variable degrees, according to the conditions of the environment.

Some authors of late have failed to understand this absolutely essential distinction, and have thus been led to conclusions which cannot hold their own before sound criticism. Even Mr. Johannsen, who, as we have seen, has proposed the terms "pure line," "phenotype" and "genotype," has not known how to apply this principle to his experiments. His studies on the effects of selection in pure lines among beans and barley, begun in 1901 and continued during a long series of years, are so exact and complete as to be a very model; yet in spite of this he has been led by them to an erroneous conclusion because he has neglected the most important principle of all. By choosing continuously through a series of years the largest or the smallest seeds to continue his genealogic families he has not been able to provoke the slightest visible change of the average constitution of the progeny in the direction chosen. And the same thing has been true in other cases.

This is in entire concord with our own conception of *indirect selection*: it is incapable of producing effects other than those caused by the enrichment or the impoverishment of the sources of nutrition. But Mr. Johannsen's experiments furnish no criterion which would enable us to decide whether the method of *direct selection* might not be capable of producing the result sought.

The study of the effects of selection in pure lines and races has evidently a double object On the one hand, then, may be a possible perfecting within the limits set by variability, the average moving more or less effectively in the desired direction. The differences may be small from the morphologic point of view, but very considerable from the practical one. An increase of 1 or 2 per cent of sugar in a field of beets is nothing to the morphologist—but it is decisive in the sugar industry. It is necessary therefore to study with extreme care small improvements of this sort. They may, moreover, lead to a widening of the extreme limits of fluctuation.

On the other hand, the studies in point bear on the question whether selection is capable of leading to a transgression with respect to the limits of the race. An affirmative response to this question has been the point of departure in Darwin's theory of selection, as everyone knows.

But my own researches have led me to a negative response, and this was what was to be expected from the grounds on which the theory of mutation is based. This view has since been corroborated by various authorities, notably by Johannsen, who has assembled a considerable collection of facts to its support. Not a single example of the production of a new genotype by means of the selection of variants of fluctuating characters has been revealed, though numerous examples of sudden mutations have been recently observed by Morgan and his collaborators among insects, and by Bartlett, Blaringhem and Klebahn among plants, without counting numerous other examples.

Here, however, it is necessary to limit the discussion to the experimental side of the problem, leaving aside all theoretic considerations. But in experimental researches a difficulty is encountered which can usually be only partially overcome. I refer to the considerable dimensions of the cultures required for the determination of hereditary percentages. Upon a square meter only 10 or 15 stalks of *Crepis* or *Dipsacus* will have enough room at their disposal to develop fasciation or torsion. In these cases 10 square meters would hardly suffice to establish an assured hereditary percentage for a single one of the mother plants. Hence if we desired to choose among a sufficient number of plants the one having a really high percentage, the dimensions of the cultures required would be much too large to permit us to repeat the selection during a series of years. Happily, however, many species demand less space, and often a choice among a small number of parents may lead to the desired result. In my selection of heads with numerous petals among the ox-eyed daisies of the harvest fields I have compared the progeny of six parents, or even less, to find one with a high hereditary percentage, and have succeeded in increasing the number of radiating petals from 21 on the average to more than 100. Indirect selection, on the contrary, of plants with the most numerous petals would very probably have given no result, as we have seen in the case of other *Compositae* through the brilliant researches of Mr. De Bruyker.

In comparing the hereditary percentages of only a few individuals much depends on chance. I, therefore, sought for characters which would permit the determination of the average constitution of the progeny of a plant during the first youth, and even when it is still in the period of germination before transplanting in the open ground. A character of this kind is found in my race of red clover having 5 leaves in place of 3. As clover does not present auto-fecundation this race had its origin in two specimens of the variety found growing wild. At the beginning the selection was made by the indirect method, but this fact hardly counted for as much with respect to improvement as can be fairly attributed to cultivation in the more favorable conditions of a garden. Then I perceived that the first leaf (after the cotyledons), which is simple in normal clover, may present, in this variety, either two or three folioles. Evidently the number of plants having compound primary leaves can be counted during germination, and we can thus determine the hereditary percentages for a whole group of plants which have furnished the seed. Among 20 or more germinators, each containing the progeny of a single mother plant, I chose that which presented the highest percentage of variants, and rejected all the others. By this means I could limit my culture each year to 20 or 30 individuals flowering. In spite of that I obtained a rapid improvement of my race, which has since been exceptionally rich in 4-leaved and 5-leaved plants, without, however, giving birth to a new genotype of a clover exclusively quadrifoliolated or quinquefoliolated.

But one rarely finds character so simple and easy, and the impossibility of auto-fecundation in clover remains an obstacle, since a detailed discussion is necessary to prove the purity of the line. For this reason I have made another choice, and I have found what I required in certain characters of the cotyledons, which can be studied in the species appropriate to pure lines as well as in those which necessitate cross or free fecundation. By far the simplest and the most frequent characters of this group are those of tricotylia and syncotylia (cotyledons respectively cleft or united). Here I will confine myself to the first group.

Tricotylia is subject to a high degree of fluctuating variability. The type specimens present three equal or nearly equal cotyledons. The less typical have two cotyledons, one of which is cleft. The cleft may be more or less deep, ranging through all the degrees between normal dicotylia and tricotylia. Some examples even present two cotyledons, both of which are cleft, or even three, of which one is more or less divided, and we find all the intermediates, even to plantlets with four cotyledons. It even happens, indeed, that one finds, in very large cultures, individuals with five or more cotyledons.

This series is uninterrupted. It comprises only the fluctuating variability of the character. We can not derive from it special races, for example, no race purely tricotylated, nor race with imperfectly divided cotyledon. Moreover, there is scarcely any parallelism between the visible character and the hereditary percentage. Apparently pure tricotylias may yield a progeny poor in tricotylia, and almost pure dicotylia may have a progeny very rich in deviations. Indirect selection is entirely ineffective here, as in other cases.

It is easy to see that the counting of the young plants is so simple as to permit a comparison between as many parents as one desires, and at each generation. We count them in the germinators, cultivated in frames in the early spring, each of which contains the seeds of a single mother plant. Among the plantlets we count two groups, the dicotylia in one group, and in the other plantlets with tricotylia and those with divided cotyledons. Only the germinator showing the highest percentage for the second group is retained, all the others being eliminated. In transplanting the plantlets from the retained germinator it is not necessary to consider individual signs of future seed bearers, though in practice the tricotylia are habitually preferred. But this has nothing to do with the true choice, which depends solely on the hereditary percentage.

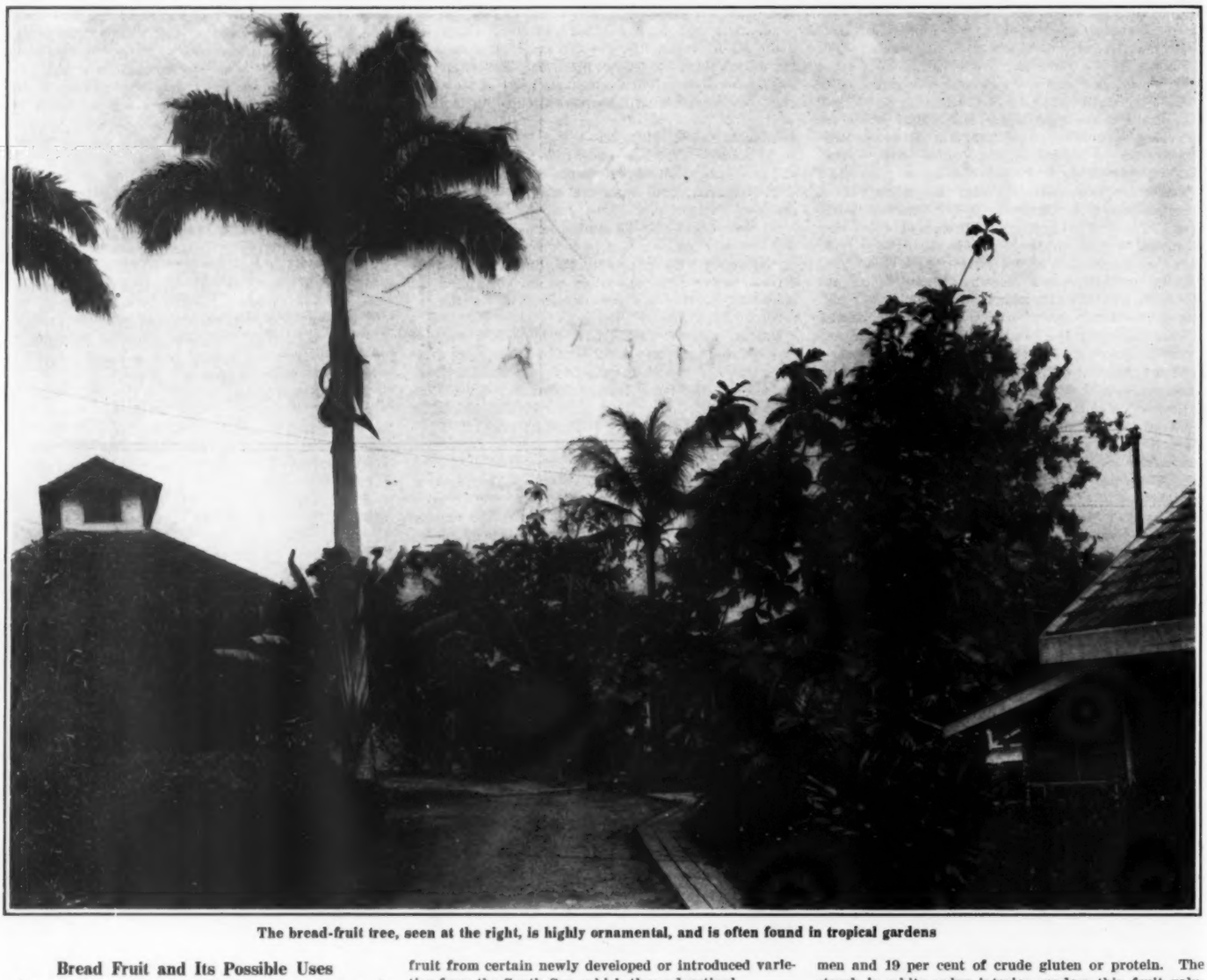
It goes without saying that the cultures must begin with accidentally found specimens. After the isolation these yield for the most part only a small percentage of tricotylia and of hemitricotylia, varying ordinarily from 7 to 20 per cent, and rarely exceeding these numbers. In the second or third generation, under the influence of experimental cultures, the proportion habitually mounts to 50 or 60 per cent, which may be considered as the normal type of the race. If now we continue the selection in the same direction we rapidly obtain 80 to 90 per cent of variants, whence the reduction, and even almost the disappearance of the dicotylias. But, in reality, the latter never disappears totally, although isolated mother plants of small progeny may sometimes seem to have a pure descendance of tricotylia and hemitricotylia. A pure race of tricotylia can not be obtained.

The results exhibited are exactly the same for the species with free fecundation, performed by insects, and for the forms of easy auto-fecundation, cultivated in pure lines.

Auto-fecundation to determine the hereditary percentages of isolated mother plants is precisely what is needed for a pure line, as it is commonly called, or for what I prefer to call a demi-pure line, because its point of departure is an individual chosen arbitrarily and not a mutant.

It is evident that a high percentage of tricotylia among the plantlets increases the chances of finding more numerous examples of extreme types, and above all of plantlets with four cotyledons, and of obtaining still greater deviations, such as specimens with five or more cotyledons. It is true that these have no significance for the selection, but if the same method of direct selection is applied to qualities which are morphologically or industrially of interest it is easy to see how useful it may become.

In closing it may be said that direct selection, which is continuous choice based upon hereditary percentages, leads regularly to appreciable improvements in the fluctuating variability of pure lines and races. But it can not lead either to a transgression of the limits of this variability nor to the production of independent new types. Indirect selection, on the contrary, investigated by many authorities, has no effect upon the improvement of lines.



The bread-fruit tree, seen at the right, is highly ornamental, and is often found in tropical gardens

Bread Fruit and Its Possible Uses

AMONG the many economically important plant products of tropical America is the fruit of a tree known in botanical language as the *Artocarpus incisa*. *Artocarpus* is the Latin generic name signifying bread fruit; the specific term *incisa* has relation to the leaves which are divided into lobes remotely resembling those of the common fig tree. The bread fruit tree belongs to one of the best defined natural families of plants which are distinguished by their aggregate fruits. By the term fruit is meant that part of a plant which consists of the ripened pistil of a flower. In the case of the bread fruit tree and its related kinds, the pistils in the female flower head in developing grow together forming a large fleshy mass resembling the mulberry in a general way. Thus, it will be noted that the bread fruit instead of being a single fruit, as it appears at first glance, is in reality an aggregation of a number of fruits, as indicated by the irregular hexagonal knobs on the surface.

This so called fruit is roundish or oval and is sometimes as much as 18 inches in length and 10 or more inches in diameter and weighs from 20 to 40 pounds. The exterior is rough and dark or sometimes glossy green; the interior is a whitish pulpy mass that has the consistency of newly baked wheat bread. Each individual part of this spongy mass, which is the result of the development of a single pistil, contains a seed called bread nut. There are from one to three hundred such bread nuts in each fully developed bread fruit. These seeds constitute an article of trade in the tropics where they are known as castañas.

Considerable interest was shown by the early European settlers in the West Indies in making the bread fruit an important article of food. When it was first introduced from the South Sea Island in 1793 into the British West Indies, orchards of bread fruit trees were planted and fabulous profits were predicted by nurserymen who based their calculations on the quick growth of the tree, the early bearing and the great yield of fruit from certain newly developed or introduced varieties from the South Sea, which they advertised.

While bread fruit was believed at one time to solve the food problem in the tropics it has never been used extensively as a regular diet. Although the fruit possesses merit as a human food, the tree has not been properly developed and cultivated. Attempts have been made to grow a few varieties in soils and sites not altogether adapted to them and the results have not been very satisfactory. On account of this lack of proper selection and breeding the plant has not been sufficiently developed so that the freshly gathered fruit can be brought to the large northern markets even by the fastest steamers. They can and are being produced cheaply and in very great abundance in all tropical countries, but the bulk of the crop goes to waste, because the varieties that are now being produced will not stand transportation. Moreover, the fruit is somewhat insipid and cannot be highly recommended so far as its flavor is concerned. The flavor is entirely different from any other known fruit and is practically unknown to the ordinary American palate. The northern traveler who visits the tropics rarely tastes the bread fruit and therefore acquires no liking for it, as he does for the alligator pear, sapodilla and other well-known tropical fruits.

There is a long list of other tropical fruit products which rarely, if ever, come into the northern markets, because they do not at present stand transportation in the condition they come off the trees, like the orange and the lemon. There seems to be no reason why the bread fruit should not become an article of commerce if the industry were properly developed and the fruits preserved in one form or another. It is possible that the white farinaceous pulpy mass in the seedless varieties may be preserved either in sugar or syrup and made into candied bread fruit. Up to the present bread fruit has not appeared in the northern markets.

Chemical analyses of the bread fruit have shown that it should prove to be a valuable food. It contains approximately 40 per cent of starch, 3 per cent of albumen and 19 per cent of crude gluten or protein. The starch in white pulpy interior renders this fruit valuable as a source of wheat flour substitute. Recent experiments made in Trinidad indicate that the separation of the starch from this fruit has proven successful. For this no expensive machinery is required. The manufacture of bread fruit meal or flour has been found practicable in any household. Other fruits and vegetable have been used in this experiment and it has been found that the cost of making the meal from such fruits as the banana plantains, figs, tannias, etc., counting labor only, varies from 1½ to 2½ cents per pound. It is claimed that the starch obtained from the bread fruit used either alone or as a wheat flour adjunct makes excellent scones. When it is used with wheat flour it makes good porridge, milk puddings, etc.

If the bread fruit tree be cared for as are other orchard trees, it makes a very rapid growth and is one of the most beautiful trees with dark green leaves and a symmetrical crown. The tree comes into bearing at an early age and lives to be very old. When grown in good ground the fruit is larger, sweeter and richer in starch than when grown in poor soil, thus showing that it, in common with other trees, does better when well nourished.

Ancient Flint Implements

AN article appeared in the SCIENTIFIC AMERICAN SUPPLEMENT for December 1st on "Flint Fracture and Flint Implements in which the author describes elaborate experiments that he had made with a view to solving the problem how the tools used by prehistoric man in the "stone age" were made, his efforts being directed to methods of mechanical chipping. One of our readers writes us that our American Indians had a much simpler way of making their flint arrow heads, which consisted in heating the edges of the stone in a fire, and then chilling by plunging in cold water. By this method he states an expert can obtain some surprising results. Possibly some such process was followed in the production of Neolithic axes and arrow heads.

A Novel Cure for Frozen Plants

Of course plants may be killed outright by extreme cold. More often than not, however, when a gardener speaks of his tender specimens as being "frozen" they are not dead at all. The effect of a comparatively low temperature on a delicate plant is to stop the absorption and transpiration of moisture. In fact, in such circumstances, the plant suffers from much the same trouble as when the conditions are very hot and dry. It has been recently shown by experiment that the very best way in which to restore the specimen to its normal activities is to stand it in a very cool place and then drench with water. The water should be quite cold certainly not more than 45 degrees Fahr. In half an hour the appearance of the plant is entirely changed, the stalks are once more erect and the foliage is spread out. The illustrations show the wonderful restorative properties of this plan.

A frozen plant under treatment

The same plant restored to activity

The Artistic Possibilities of Concrete

THE principal objections to concrete from the architectural standpoint have been its lifeless color, its monotonous appearance and the limited scope which it offers for variety and beauty of form. These objections, however, based as they are on limited experience, are not really justified, and the frequency with which they are uttered is clear evidence that, though the value of concrete as a structural material is now firmly established, its possibilities as a medium for the expression of artistic feeling have hardly yet begun to be realized.

Traditional construction has consisted in a collection of units, assembled according to the design of the architect, and the design has been prepared with due regard to the limitations of the material employed, viz., timber, brick or stone. Concrete construction, on the other hand, involves an essential departure from the traditional, and by presenting new problems for consideration must exert a corresponding influence upon design.

The chief characteristic of concrete as a structural material is plasticity, and this characteristic renders it adaptable to any design, and completely subservient to the will of the designer. If this is fully realized it will be seen at once that so far from offering a limited scope for variety and beauty of form, concrete construction affords unusual opportunity for exploring new fields of architectural treatment.

Much of the work so far done in concrete, though not all by any means, has been in great engineering undertakings where strength has been the sole desideratum, and where beauty has not been considered; but strength and beauty are not incompatible, and there is no reason why, as is too often the case, great strength should be associated with unattractiveness or even positive ugliness.

Concrete, being monolithic, lends itself to treatment in either large masses or small; but in either case the keynote of concrete architecture should be truth, and truth is simplicity. This, however, does not mean that ornament is precluded, far from it; but it does mean that concrete should not pretend to be brick or stone, nor, indeed, anything that it is not, but frankly concrete. Hence, for example, the marking of a concrete surface to imitate courses and joints is not only illogical but is a departure from the truth. For purposes of this article, concrete block construction, being more or less a form of masonry, is not included as a form of concrete architecture in the sense in which it is being considered here, although this method offers ample scope for artistic treatment.

Next to the general design of a structure, the character of its surface is its most prominent feature, and the one which has the greatest effect on its appearance; and herein probably lies the weakness of most of the earlier efforts in concrete as a structural material. The artistic instinct demands a broken surface, whereas the efforts of the concretor are largely directed towards the production of a surface as smooth and uniform as possible; hence that monotonous similarity which calls forth the criticism so often leveled against the appearance of the concrete building. But the fact is lost sight of, or imperfectly realized, that concrete construction lends itself to a large variety of forms of surface treatment, and there is no doubt that this constitutes one of the most important elements in the architectural possibilities of concrete as a building material.

Thus there may be said to be two distinct schools of concrete design, the one pinning its faith to the smooth stone like surface, and the other advocating such treatment as will satisfy the feeling which seeks expression in color and texture. But the truth is that both these methods are legitimate, and, since they are respectively adaptable to different classes of building, each has its place in the wide field of concrete design.

Several methods are available for producing the effect of broken surface combined with color, and these may be divided into two classes. By methods of one class the outer film of cement mortar is removed and the desired effects obtained by the exposure of the aggregate, and the methods of the other class consist in adding selected material to the surface either during or after concreting. Delightful effects can be produced by either of these methods, but to the purist the former will appeal the more strongly as being more consistent and the more frankly concrete. These types of surface may now be considered in turn.

By carefully selecting the aggregate with regard to color, size, texture and proportion, almost any variety of surface may be obtained by suitable treatment. Whatever be the aggregate that is used, and in whatever proportion it is employed, if the surface is left untreated the general effect will be practically the same, since the cement mortar will flow round and coat the larger particles of aggregate and present the uniform grey color to which objection is raised. In order to expose the aggregate and bring out its richness and variety of color, this outer film of mortar must be removed. This may be accomplished either by brushing the surface while the concrete is green or by tooling or sand-blasting after it has hardened.

Where the surface finish is to be obtained by brushing it is necessary to remove the forms as soon as possible, but care must be taken that the brushing is not commenced too soon, or particles of the aggregate will be torn out, leaving unsightly pits. An ordinary scrubbing brush with stiff bristles will serve the purpose if the concrete is green, but if it is at all hard a wire brush will be necessary. During the brushing, water should be freely used so as to leave the aggregate quite clean and thus obtain that purity of color which gives this type of surface its peculiar charm.

If it is found that the color of the aggregate is not fully brought out by brushing, the surface may be scrubbed with a dilute solution of muriatic acid (1 part of acid to 3 or 4 of water). The work should be gone over rapidly and the acid immediately removed by the liberal application of clean water, preferably by means of a hose.

Suitable aggregates for this purpose are red granite, different colored gravels and sands, chippings of various colored marbles, or combinations of these in various proportions.

This process, however, has its limitations. In the case of a building it is not usually possible to remove the forms until the concrete is hard. When this is the case another method may be adopted, namely, that of bush hammering. A bush hammer, briefly, is a hammer whose face is cut into points or teeth. This cuts away the mortar, exposes the aggregate and leaves it standing out in relief. Bush hammering together with the application of acid produces the same effect as brushing.

The delightful effects which may be obtained by these methods well repay the time and labor expended upon them.

We may now consider methods of the second class, namely, those by which particles of selected material are added to the concrete. Under this heading stucco and its allied processes might be expected to be included, but as this is a separate subject, considerations of space forbid further reference to it here.

Where the cost of a specially selected aggregate would prohibit its employment for the whole of the concrete work of a building, the mixture may be applied as a facing material only, and may be from one to two inches thick, the backing being the ordinary concrete for the wall. This facing material is applied to the surface of the wall-form immediately before the concrete for the backing is placed in position. The tamping in this case should be mainly on the backing, so that the particles of coarser aggregate in the special mixture are forced outwards, and the backing rammed into the facing, causing the two thicknesses to form a homogeneous mass.

Another method of applying the facing material is by the use of iron sheeting which is placed in position as a partition or intermediate form inside and parallel to the outer wall-form, and distant from it from one to two inches according to the proposed thickness of the facing. The height of this sheeting should not exceed one foot, or a difficulty will be experienced in depositing the material within the narrow space, and angle irons or other means must be employed to keep it in position while the concrete is being placed. The space between the metal form and the front of the wall-form is filled with the facing mixture, and the back with ordinary concrete. The metal sheeting is then withdrawn by means of handles attached to it for the purpose, the backing tamped against the facing and the process repeated for another section. After the forms have been removed the surface may, if necessary, be treated by washing or tooling.

For interior work the flat concrete surface may be further enriched at suitable points of accent in accordance with the recognized laws of design, by mosaic patterns worked out in colored marbles, burnt clays or other material appropriate to the character of the building and in harmony with the color scheme selected. Such patterns may be laid in pre-cast slabs or tiles which are placed next the face of the form and incorporated into the wall as the concrete is placed in position; or recesses may be cast in the surface of the wall and the slabs cemented in after the forms have been removed; or again, the particles forming the pattern may be attached by an adhesive to sheets of paper which may be fastened to the face of the form in the desired position and the concrete filled in behind them. In this case the surface would require to be dressed after the forms have been taken down and the paper stripped off, as some of the cement mortar would flush to the surface and appear on the face of the pattern.—*The Architect.*

Sugarless Candy

INGENIOUT experiments are being made to save sugar in the preparation of confections in which the sweetening is supplied in other ways. Honey, molasses, maple syrup, corn syrup and other things are used to take the place as far as possible of the fine confectioner's sugar formerly available in practically unlimited quantities. In fact, the amount of sugar that could be bought in the past was only restricted by the amount of money one desired to invest.—*Sweets.*

Some Reforms in the Patent System*

An Important Institution Which Should Be in a Continual State of Evolution

By F. W. H. Clay, Assistant Commissioner

It is highly praiseworthy in the Examining Corps that it has of late evinced a desire of self-criticism and reform in its work, for when one has learned honest self-criticism he has made a good start up the hill to wisdom. I speak primarily as one trained as an attorney, although now taking some small part in the administration of the Patent Law. But I think there should be no difference between the points of view of the attorney and the examiner. In a sense we are all examiners; in a sense we are all attorneys. We are all alike interested in the same great system of promoting industrial progress by invention, and in that best branch of the noblest of professions, the law of property in ideas. Our work lies in the very center and well-spring of evolution of science and industry. We attend the birth of new ideas of applied science. We are the familiar associates and helpers of creative genius.

Because the Patent Office is the nursing place of industrial evolution and an institution for measuring the place and fixing the rights of these steps of progress, it ought to be itself always in a state of evolution. The law which in part it creates and in turn is governed by also must be always in a state of evolution—the never-ending process of sloughing off the obsolete and the erroneous, and of preserving and developing the good and the true. The first step in this process is of course the discovery of those errors which are not always so obvious.

No man expects to be a successful gardener without waging an endless war against weeds. The old familiar ubiquitous weed of red tape troubles the patent system, as it impedes the progress of every other national institution. And of course there is always some delay, slack motion, injustice, and error in the conduct of the business of the Patent Office. It would be a miracle if this were not so; the very amount of business done makes it unavoidable. In a year's time some seventy thousand applications for patent are examined and each one compared against three million previously granted patents; over forty thousand patents are issued and classified; over a thousand interference contests are conducted and decided. The United States Patent Office probably has double the amount of work of any foreign patent office and one-third more than that of the whole British Empire. And the entire labor of examining and classifying inventions is carried on by a force of only about four hundred men.

But aside from the inevitable faults resulting from the very complexity of the patent system, there are specific ills which have long remained without cure, and it is for the purpose of calling wider attention to these that this paper is written. And first, to mention some defects that have only lately been corrected

The widest and most severe criticism heard is of the delays in the issue of patents. It should be more generally understood that the chief reason why some inventors are denied prompt action on their applications is that other inventors insist upon themselves delaying action in their cases. Commissioner Ewing made a heroic and successful effort to improve this condition by forcing out of the office much of the great accumulation of long delayed cases. Whereas in 1914 there were nearly 4,300 applications still pending after five years of prosecution, there are now not over 1,600—a reduction of about 63 per cent. Cases pending longer than fifteen years have been reduced to not more than a sixth of the former number. At the present the average period of prosecution is about twenty-one months, and 97 per cent of all applications are disposed of in less than five years.

Greater than the business advantage of reducing the accumulation of laid-over work and conducting business promptly, is the moral advantage of definitely establishing a policy of recognizing the public's interest and its right to prompt dealing with patents. The long prevailing idea that it was solely the concern of the applicant how long he kept his application pending must be removed. The deliberate holding back of the date of appearance of a patent until after the art subject to it had grown up by the efforts of others had become a national scandal; and this practice is now in a fair way to be stopped. The interests of the owner of the invention are protected by prompt grant of a patent and the public is also protected by advancing the time of disclosure and beginning of the monopoly term.

*A paper read before the Examining Board of the U. S. Patent Office.

The moot interference had long been a bugbear to the office and a dangerous tool in the hands of unscrupulous business men. It was a common practice a few years ago to prevent or delay the issue of a patent by bringing about interferences with it for the sole purpose of blocking progress by an intricate system of legal technicalities. Commissioner Ewing succeeded in avoiding the declaration of interferences between two parties where the papers filed in the Patent Office show that there is no chance for the later applicant to prevail upon the merits of a claim of priority. Against a long settled practice, and against the opposition of many prominent attorneys, and even of the two courts immediately above him, the Commissioner finally succeeded in securing the right to prevent these contests of priority of invention from being turned into mere selfish opposition to the grant of a patent to another. (See Ewing, Commissioner of Patents, v. The United States, *ex. rel.* The Fowler Car Company, 238 O. G., 983, decided May 7, 1917.)

The costly delays of useless interferences had been prior to 1914 further aggravated by irregular and uncertain processes of conducting them. It was the fault of poor organization and of scattered jurisdiction. There were 43 different judges as to whether and when an interference proceeding was necessary; and there were 43 different judges of the question whether an interference once started should be dissolved without a judgment of priority. The result naturally to be expected is shown by the fact that in the year 1915, for example, it was found that nearly a third of all the interferences proposed by primary examiners ought not to be instituted at all, and nearly as many more needed essential revision of the issue to be tried.

By concentrating the function of deciding these preliminary questions in one man under the direct guidance of the Commissioner and in direct consultation with all the expert primary examiners there has been brought about a stable policy and a uniformity of judgment which cannot but prove of great value and result in great economy of labor.

Similarly, there has long been a serious evil of dividing up into several patents related inventions which ought to be patented together, and a great irregularity of practice resulted from having 43 independent judges of when division was necessary. Since 1914 the decision of all cases has been put in the hands of one law examiner, who acts after consultation with the particular primary examiner most familiar with the invention in each case. This has resulted in great saving of labor and expense, and prevented a harmful multiplication of patents. The several improvements just mentioned have as one of their main objects to allow the primary examiner better opportunity for the performance of his own proper function of determining patentability. The decisions of all matters of law and policy have been concentrated in the hands of the Commissioner and the law examiners.

Besides these evils, now in a fair way to be eliminated, there are other important evils which ought not to be allowed longer to continue. Some of them also have grown to the proportions of a national scandal, and by their very magnitude have acquired great power to combat any attempt to correct them. Among these are the condition of the classification of inventions; the uncertain scope and obscure form of patents, and the matter of the ethics of the fraternity of patent attorneys.

When it is remembered that the object of the classification of inventions is to make available the accumulated knowledge provided by inventors in the whole field of the useful arts, no argument is required to show the importance of this work. When it is stated that the present reclassification has been going on for twenty years and is not yet half done, it will require no demonstration that something is wrong, either with our theory or with our method of procedure; and this after making due allowance for the notorious fact that the office force and facilities are inadequate.

The classification developed up to the year 1898 by a sort of sub-conscious selection by many skilled minds. It was a natural and a popular classification, but it had become very confused and illogical. As all sciences have a tendency to depart from the approximate and seek for the exact, so it was with the new classification started in 1898. There has been a tendency to the abstruse and a tendency to make the classification an end in itself.

The work of the Patent Office is in the main a comparison of alleged new ideas of means with what has been done before. In order to determine novelty a knowledge of what is old is not only requisite, but also is evidently impossible to gain without an arranged, defined and accessible set of references. This set of references is the public instrument for testing the validity of patents; and it is the examiner's principal tool in his daily work of inquiring into patentability. A visitor from Mars may be astonished to hear that we had worked for twenty years with an unfinished tool, and might ask why we had not dropped everything else until the tool was finished. This is not a just criticism, however, because as a matter of fact the classification must be a continuing process. It can never be finished. It deals not only with things now known but must continually contemplate things hereafter to become known. For example, it is hardly conceivable that when the phonograph was invented there could have been an already established class of machines ready to receive it. But certainly it is a fair criticism that the present system of classification has been unduly delayed in bringing it to a working state, and that this is one of the causes of difficulty in keeping up the examining work.

It would be unwise to suggest off-hand any radical remedy for defects in the system, and particularly before making a correct diagnosis of the defects, which obviously is extremely difficult. But many think that the principle and theory of the classification is perhaps too refined, and again that the method of the work is not logical because the classification is not in close enough touch with the examiners who are familiar with the arts. Personally I think the classification has been prosecuted too much on the basis of the patent as a patent and not enough on the aspect of the patent as a publication or disclosure. The classes of invention ought to accord more nearly with the publicly understood definitions of the arts as they have actually developed in business. The primary examiners are probably more capable of carrying on the classification in this way. Of course there must be a supervision by the classification division, in order that all the examiners may classify upon the same general principle and harmoniously with each other. All students of the matter will probably agree that the classification can be made a more workable tool of search in the hands of men of ordinary skill in searching by emphasizing the point that the classification is not an end in itself, but is a means to an end, and that it should be a tool for the average rather than for the exceptionally learned or skillful examiner or attorney. The need of reform has been emphatically urged by the great mass of attorneys, and it would be better to make even a risky attempt at certain radical changes than to remain in a state of apathy, continuing to trust solely in the hope that Congress may provide a force large enough to speed up the work to completion.

I think much may be done in the aid of classification by improving the form of patents themselves, and here every examiner may assist every day. In fact the principal difficulties of the classification as it has heretofore been carried on arises out of the defects in the patents themselves. The original plan of the classification by novelty was based on the supposition that the patent was written—as it should be written—so as to disclose no more of the old art than was necessary to explain the new addition thereto, and so as to distinctly claim the part which was new. It did not contemplate aggregation claims, nor supernumerary claims nor a disclosure of more than the essential invention and its necessary setting. But the fact is that a great many existing patents fall far short of this ideal condition, with the result that they can only be placed in proper classes by a system of cross-referencing which is extremely complicated and cumbersome. Anything that can improve the form of patents must be a potent aid to the classification. The two reforms are intimately bound up together. As a single example, if each claim of a patent not only made a clear distinction and pointing-out from the prior art, but also was clearly distinguished from any other claim of the patent, then the classification on the basis of claimed invention as originally designed could be actually carried out without great difficulty.

The form of the patent has of late years become more

and more complicated and more and more technical. This is partly due to the modern custom of large corporations employing regular corps of patent attorneys who grow to be experts in their particular arts and are apt to look at everything from the expert's point of view. It has perhaps not been sufficiently remembered that the primary design of a patent is to inform the public how to use new arts and machines. Every patent should contain somewhere a description of the operation of the invention in such simple terms that he who runs may read. While of course the patent is addressed to those who are skilled in the art, it nevertheless remains that the patent is an instrument for public instruction and its primary function is to impart new knowledge in usable form.

Secondarily the patent is a charter of monopoly. In providing for this function there has arisen a very serious evil of which the courts have long made complaint. Simple as is the requirement of the law, that the inventor shall particularly point out what part of his disclosure is his own invention and what part is not, both the public interests and the inventor's interests are frequently jeopardized by making claims of the invention in great numbers and with great varieties of expression, so that the net result is not a pointing out of the invention, but a carefully laid net to catch infringers who may hereafter use unexpected modifications of the invention. The patentee may safely leave it to the court to protect his invention; his duty is to point out just what his invention is. It is indeed astonishing that the evil of supernumerary and indefinite claims could have been allowed to grow to such proportions as it has reached. If one were asked which fruit he preferred and replied by naming a half dozen fruits the absurdity of the reply would be patent. Yet this is exactly parallel with what patentees are continually doing. It is common practice to claim the same invention in numerous degrees and as separately aggregated with numerous other elements, and in numerous forms of language. A reform is necessary in the public interest. It should be made perfectly clear that one in preparing his charter of monopoly is bound to avoid all confusion and uncertainty and to mark out the boundaries of his forbidden field with distinctness. The invention actually patented should be pointed out in a few words confined to the essentials, and should be pointed out but once.

So far as future patents are concerned, I repeat that if their disclosures could be confined to the essential inventions to be patented and their claims made definite and distinct, this would solve at once many of the difficulties of the great work of classification.

The Patent Office ought to exercise as much authority to keep up the moral tone of its bar of attorneys as the courts exercise over their bars. The present is a peculiarly opportune time to undertake a reform in the ethics of the profession of patent law. The examiners ought to call to the attention of the Commissioner all lapses from propriety.

As a class inventors are among the most gullible of men. An inventive genius is quite generally an enthusiast, and it is the disposition of an enthusiast to believe all things. Inventors are not found exclusively among those actually employed in the great manufacturing industries, nor in the great centers of population, where men are surrounded by keen witted lawyers and business men. Many of them are in far outlying country places and in obscure positions where they have no opportunity to obtain reliable legal advisers. These men see in the papers plausibly worded advertisements of patent attorneys and obtain from them laudatory circulars, apparently showing that the attorneys are highly recommended, and they forthwith trust their money and welfare to men they have never seen. Just because they are a peculiarly easy prey to the unscrupulous attorney a patent attorney ought to be, even more than a general practitioner, a man of integrity and fair dealing.

The present evils are glaringly apparent. Make-believe inventions used for promoting stock selling schemes are usually carried on by the aid of unscrupulous patent attorneys. These should be vigorously searched out and eliminated. Great numbers of incompetents who have prostituted a profession into a mere business of fleecing the innocent, swarm about the Patent Office offering alluring prospects to useless ingenuity. When they catch the innocent they file imperfect papers; they prosecute with the sole view of getting a patent quickly without regard to its value; they habitually induce inventors to file expensive foreign applications before investigation as to whether there is an invention present. They encourage efforts in wrong directions, promote the patenting of immature ideas, waste the money and energy of inventors, all for the sole purpose of swelling their own profits.

While it is a heavy and thankless task to undertake to improve this condition, it is nevertheless a reform that can be and must be accomplished, and the examiners can do much to accomplish it. Specifically, I think a carefully selected committee of the patent bar of the whole country should be called on to prepare a clear-cut code of ethics, and thereafter, with the aid of the better element of the bar, the Commissioner should sternly apply the code. It would be to the great benefit of the public, increase the popular respect for patents generally, lighten the labors of the office and the courts, and lessen the chances of wildcat promotions. It is due to the inventors that they should be defended by the institution to which their welfare is confided by the law.

Finally, there is the great and ever present problem of separating the executive and judicial functions of the Commissioner. The main obstacle is that substantially the same training and special qualifications are requisite for the performance of each of these functions. This arises out of the peculiar nature of the property in inventions, and of the business of the Patent Office.

It is indeed somewhat of anomaly that in a government fundamentally based upon the theory that judicial and executive functions must be completely divorced, we have an institution in which apparently there can be no such divorce. But with a larger and better organized force it might be quite possible to substantially separate them. At the present time, as we all know, the Patent Office is undermanned. badly so; I estimate that it is at least 25 per cent. undermanned for proper performance of the duty of examining applications. It is quite impossible for any commissioner to confine his attention either to the judicial function or to the executive function. He may delegate parts of each function to others, but eventually must be responsible for both.

There might be some substantial reform in the direction of sub-dividing the labors so that the commissioner's judicial function would be largely taken up with questions of law. Possibly the time will come when the Supreme Court will be enlarged and have a section specially designed to review those cases involving the law dealing with the rights of property in ideas; and by periodic selection for a limited term of service from among the judges all over the country a court could be formed which would have a point of view on patent matters which, while stable, yet at the same time represents the variant opinions of men in various parts of the country and in various conditions of life. When that time comes the appeal from the commissioner may be direct to the Supreme Court, and the commissionership will be an office of such dignity and emolument as to command the services of the very best men—men of wide experience and learning in law and science, and of judicial temperament as well as executive ability.

Furthermore I expect to see the time come when in some way the commissioner will be virtually a law judge and the Board of Examiners-in-Chief will be a refined sort of jury, to pass upon the facts of invention. The appeal in interference cases, involving as it does most frequently questions of law, might well go direct to the commissioner, leaving the board only the function of dealing with the propriety of the grant of patents on their merits. A body of trained and learned men having studied and passed upon the question of invention, in my opinion their decision ought not to be reviewed and reversed by any one man, unless in clear case of an error of law.

All these suggestions, however, are merely suggestions. I make them only to illustrate my main point, that the Patent Office is a great and important institution whose organization and work must be in a continual state of evolution, because it deals with property and law in process of evolution. The immediate duties before us are to make more uniform the work of the various divisions, to improve the form and contents of the patent as a legal paper, and to eliminate all unnecessary work so that we may, as Mr. Ewing often expressed it, do a year's work within a year the best we can with the inadequate equipment the Congress has seen fit to give us.

Motor Fishing Vessels Use of Oil Engines

THE marine internal-combustion engine has had a strong influence upon the fishing industry during the past few years, not only in Great Britain and America, but also in Japan, and perhaps more particularly in the Scandinavian countries, which were almost the first to see the advantages of installing oil motors in fishing vessels.

Since the fishing vessels of the world are not all classified, it is not possible to give complete details of progress in the different countries. But from such annual reports as are issued it is possible to gain a very clear idea of the impetus which marine motors have given to the fishing industry and the important stage which the movement has now reached. For instance, round the coast of Scotland, whereas as recently as 1908 there were not a dozen fishing vessels in which motors were installed, at the present time there are well over 1,000 such craft, while during the last 12 months more than 200 motor fishing vessels have been built or motors installed in existing sailing boats. All round the coasts of England, motor fishing vessels are being constructed where circumstances permit, and oil engines installed in the present sailing vessels where possible. It is now no longer a question of belief or disbelief in the advantage of the marine motor for fishing vessels, but merely a problem of the possibility of obtaining engines at the present time or of getting new boats built. The prosperity of the fishing industry in recent years, coupled with the fact that fishermen have now a confirmed belief in the advantages of the internal-combustion engine, has led to the construction of larger vessels than were usually common; and the majority of the new fishing craft that are built, or the sailing boats that are converted to auxiliary power, are comparatively large—that is to say, well over 45 feet in length. At the same time, the number of steam-driven fishing vessels is actually on the decrease, although for the largest type of trawlers steam machinery is still for the most part employed.

CHARACTER OF ENGINES

The engines installed are either of the paraffin electric-ignition type or are hot-bulb motors in which the ignition is effected by means of a hot-bulb, heated by a blow lamp for starting up but kept at the proper temperature during the operation of the engine by the heat of compression within the cylinder. It is satisfactory to note that in most cases British engines are being and have been installed, such types as the Gardner and Kelvin being perhaps the most common. On the other hand, the price of paraffin fuel has increased so considerably of late that the advantage of the hot-bulb engine has become more marked, and fishermen will undoubtedly show a preference for this type, using a heavier and cheaper fuel unless the selling price of paraffin decreases to a marked extent within the next year or two Already a fair number of hot-bulb engines have been installed in fishing vessels, the majority being built in Scandinavia, since such motors are not so largely constructed in Great Britain. One or two British types have, however, been employed, and it is possible that British manufacturers will have to turn more attention to the construction of the semi-Diesel engine if they wish to retain their complete hold upon the fishing industry in the future.

There is a wide field for the application of the Diesel motor in larger fishing vessels, and particularly in trawlers. Up to the present time comparatively little has been done, owing to the difficulty that has been encountered in devising a means for driving the trawl winch. This seems now to have been overcome, and it is not unlikely that there will be a big development in the construction of large motor trawlers for service around the shores of this country. There are other still more interesting applications of the oil engine to large fishing vessels: in particular, the construction of several high-speed vessels built for the purpose of bringing fish from the Newfoundland fisheries direct to this country. The scheme is an ambitious one, but there is no reason why it should not turn out to be a complete success.

PROGRESS ABROAD

The increase in the employment of motor fishing vessels in other countries is not less marked than in Great Britain. In Japan there are enormous numbers of fishing vessels, and whereas less than 10 years ago not one of them was equipped with an oil engine, there are now over 3,000 so fitted. So important has this development become that a Japanese engine-building firm has now undertaken the construction of marine motors on a large scale, and is capable, it is said, of turning out several hundred engines annually.

In America practically nothing but motor fishing vessels are being constructed, and the same is true of Scandinavia, where steam-driven craft are almost unknown and sailing vessels without auxiliary motors bring in such poor returns that they are unprofitable to run in competition with the motor or auxiliary vessel.—*Engineering Supplement of the London Times.*

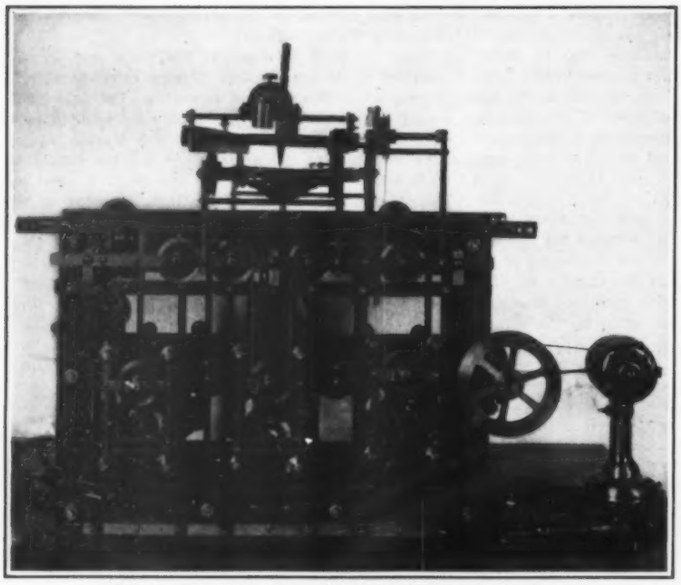


Fig. 2. Front view of machine, showing pen mechanism

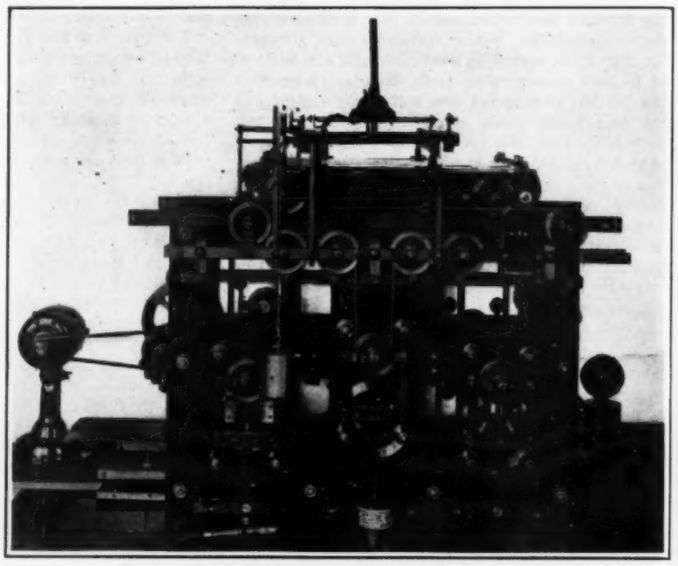


Fig. 3. The machine arranged for rectangular curves

A Compound Harmonic Motion Machine—I

Adapted for Drawing Any Predetermined Curve With Mathematical Accuracy

By William F. Rigge

HARMONIC motion machines always possess special attraction for a physicist on account of their numerous applications in theory and in praxis. They appeal also to the mathematician, as they make whole series of complicated curves amenable to similar and simple formulæ. By reason of the infinite variety and surprising beauty of the figures they produce, they offer endless enjoyment to the amateur.

The object of this article is to present a new and comprehensive harmonic motion machine. It may look very intricate on first inspection and may call for great mechanical skill and labor in its construction, but it makes use of a very simple principle, lends itself very successfully to the instruction of students, and draws an endless variety of curves.

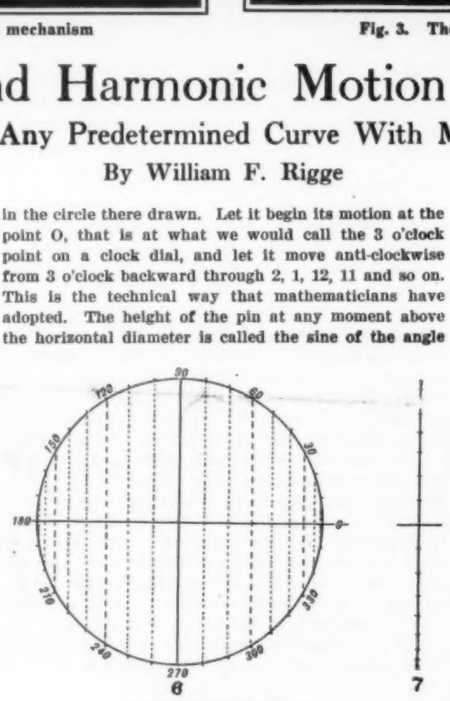
PRINCIPLE OF THE MECHANISM

Fig. 1 will introduce us at once into the vital principle of the mechanism. From the center of the graduated circle an axle protrudes and carries an adjustable crank pin at its end. This pin is screwed into a square rod sliding into a square tube, both ends of the rod being rounded and threaded so that by means of opposing nuts the crank pin may be set at any distance from the center. As the axle revolves, the crank pin moves in a circle, and by sliding in a slot between two square bars, it raises and lowers a rectangular frame, which is held to a vertical course by means of four grooved rollers bearing against two parallel rails. A cord passes over a large grooved pulley in the upper part of the rectangular frame. One end of the cord is anchored to a firm support, while the other gives motion to a carriage holding a drawing pen against the opposing pull of a weight.

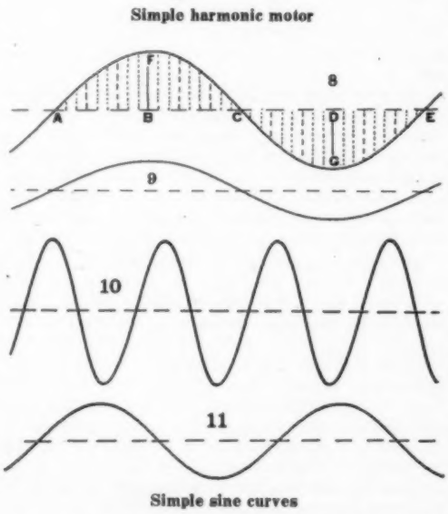
In untechnical language, we see that it is only the up-and-down motion of the crank pin that is communicated to the cord and through it to the pen. In mathematical parlance, we would say that the vertical distance of the crank pin from the horizontal diameter of its circle is the radius of this circle multiplied by the sine of the angle that the crank makes with this diameter. As the grooved pulley in the rectangular frame is a movable one, the motion of the drawing pen is always exactly double that of the pin, so that only the extent, but not the character, of its motion is changed. The distance of the drawing pen from its mean position is then always as in Θ, a being twice the radius of the crank pin and Θ the angle of the crank at the moment. This oscillatory motion of the pen is called simple harmonic. It is that of the points of a vibrating string giving its fundamental note without overtones, and this is the reason of its name. It is also like that of a pendulum and of many other vibrating bodies.

SIMPLE HARMONIC MOTION

Fig 6 illustrates this simple harmonic motion in a graphic way. Let us imagine the crank pin to revolve in the circle there drawn. Let it begin its motion at the point O, that is at what we would call the 3 o'clock point on a clock dial, and let it move anti-clockwise from 3 o'clock backward through 2, 1, 12, 11 and so on. This is the technical way that mathematicians have adopted. The height of the pin at any moment above the horizontal diameter is called the sine of the angle



Simple harmonic motor



Simple sine curves

that the crank then makes with this diameter. This sine is zero when the crank pin is at zero. It grows at first as rapidly as the pin moves, so that at 30 degrees it is already equal to half the radius of the circle. Its growth is, however, diminishing in rapidity more and more, until when the angle is 90 degrees, the height is a maximum and equal to the radius of the circle, but the growth has ceased entirely. The sine then decreases, slowly at first, and then more and more rapidly, in the very same, but reversed, order in which it grew. At 180 degrees, the sine is again zero. It then sinks below the horizontal diameter and is minus, until it reaches its greatest negative value at 270 degrees, from which it again diminishes arithmetically to zero at O degrees.

If the crank pin revolves at a uniform speed, its vertical position at 10 degree intervals is that shown in Fig. 7 (or even in the spacing of the sines on the horizontal diameter of Fig. 6), in which the outer or end positions are crowded together. In fact, Fig. 7 is what the shadow of the points on the circle in Fig. 6 would look like when the paper is held edgewise to the sun. This rhythmic motion of a point, taking the consecutive positions shown in Fig. 7, is called simple harmonic. As these positions are spaced at equal angle intervals and at equal time intervals, they represent the successive positions of points on a musical string or on a pendulum in equal fractions of a vibration. The proof of this truth is algebraic and is not readily grasped by the average student or by the untechnical reader. This is the chief reason why the writer has preferred to use in his machine the geometrical principle of a revolving crank pin rather than the algebraic one of a swinging pendulum, though the latter is almost universally employed in machines of this sort. The geometrical principle emphasizes the momentary *position* of the drawing pen, the algebraic one considers its *acceleration* or variation of speed. For this reason in the present machine the pen may be run at any speed, it may even be stopped at any point and made to reverse its course, a proceeding manifestly impossible with a pendulum.

ELEMENTS OF SIMPLE HARMONIC MOTION: PERIOD, AMPLITUDE AND PHASE

An analysis of the nature of simple harmonic motion discloses three elements: the period, the amplitude and the phase. The phase in the present machine is simply the angle Θ of the crank pin. This is indicated on the graduated circle in Fig. 1 by a pointer under the crank and not visible in the figures. The zero point of the angle is at the right end of the horizontal diameter, and the motion of the crank, as one faces the circle, is always anti-clockwise, as is the law in trigonometry. A clockwise direction would have served the mechanical purpose equally well, but as the instruction of students was the first object of the machine, the usual mathematical conventualities were always kept in view.

The second element, the amplitude, is the distance the pen may move away from its mean position. This distance, as was mentioned before, is twice the radius of the crank circle, and is adjustable within the desirable practical limits and with the required accuracy.

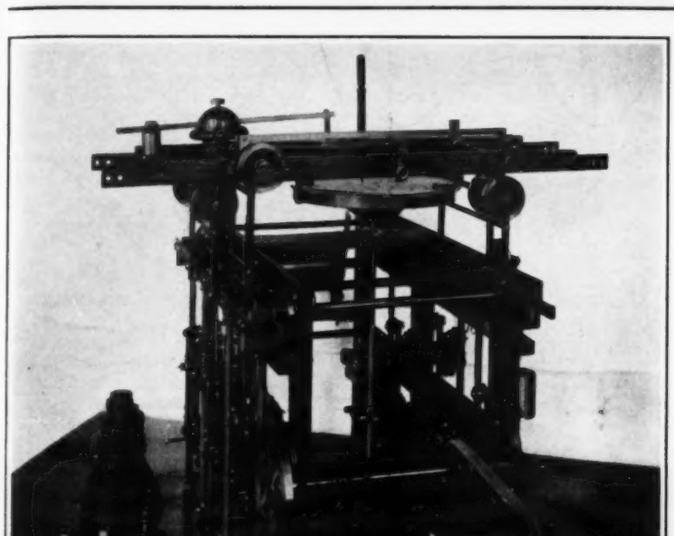


Fig. 4. Interior mechanism, and description of polar curves

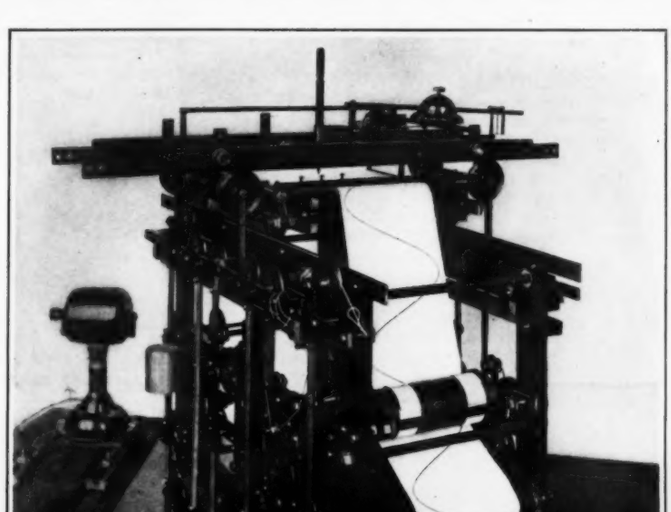


Fig. 5. The machine arranged for sine curves

The third element, the period, is the time required for one complete vibration, that is, for one complete motion of the pen to and fro, or for one complete revolution of the crank pin. The period is made to depend in the present machine upon a gear wheel on the inner end of the axle. Fig. 4 shows how this is done. A small electric motor in the foreground by means of a rubber belt sets in motion a cross shaft that carries a fly wheel at its left end. Two endless screws on this cross shaft, one right and the other left handed, gear into 16 cogwheels at the ends of two long parallel drive shafts, each of which carries three endless screws gearing directly into the wheels on the inner axles of the cranks. Fig. 1 shows a vertical slot in the side of the machine in which the journal can be fastened to allow for the varied size of the wheel and to throw it in or out of gear. This journal consists of a short piece of ⅝-inch iron pipe carrying the graduated circle at its outer end and threaded on its inner end, and held in its place by a nut and washer. The period is then inversely as the number of cogs on the gear wheel, that is, the greater this number, the smaller the period. At present there are 14 sizes of wheels, having 16, 20, 24, 28, 30, 32, 36, 40, 44, 48, 52, 56, 60, 64 cogs, with duplicates of many of them.

GENERAL VIEW OF THE MACHINE

Fig. 2 gives a broadside view of the whole machine. The strong oak framework is 20 inches long and 14 inches high. It presents three units or sections, which are alike in all details, and thus shows the reason of the word "compound" in the name of the machine. The left unit F is idle at present. A cord anchored at the upper end of unit F passes over two idle pulleys down and around the movable pulley in the middle element E, and then up and down and around that of element D to the right. It then runs upward over a fixed pulley and is fastened to the pen carriage, as may be seen in Fig. 4. From near this same point, we may trace another and shorter cord going to the right and down to the weight which keeps the cords always taut. The fact of the first cord's passing through two units in Fig. 2 shows that the motion of the drawing pen must be the resultant of the algebraic sum of the simple harmonic motions of both. The cord might also be drawn through the unit F, and, instead of being

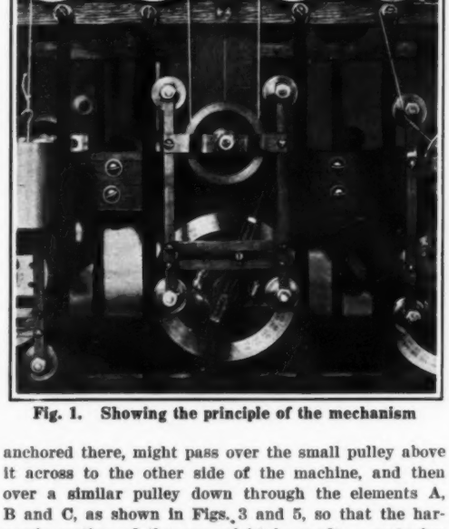


Fig. 1. Showing the principle of the mechanism

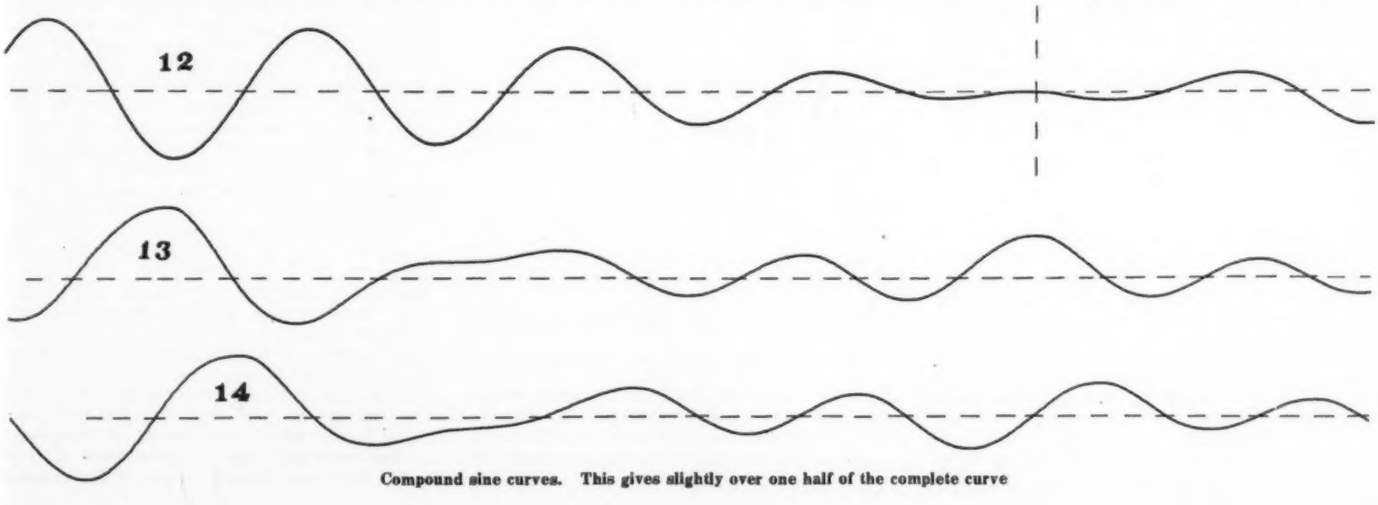
anchored there, might pass over the small pulley above it across to the other side of the machine, and then over a similar pulley down through the elements A, B and C, as shown in Figs. 3 and 5, so that the harmonic motion of the pen might be made a sextuplex combination. A quadruplex combination has actually been tried on the machine and will be shown later. In principle there is no limit to the number of these combinations, and the United States Coast and Geodetic Survey uses as many as 36 units in its tide-predicting machine (*Popular Astronomy*, XX, 269).

THREE WAYS OF MOVING THE PAPER

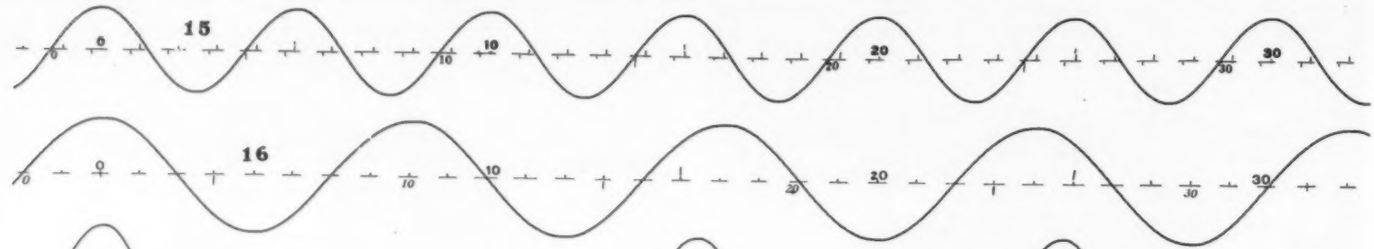
The simple or compound harmonic motion imparted to the drawing pen by one or more of the units in operation, carries it continually over the same straight line in Fig. 7 and would be of no practical utility unless some sidewise motion were also communicated to the pen or to the paper under it, so that the vertical position of the pen at any moment could be definitely recorded. In this machine, as in others generally, it is the paper that is moved in a direction at right angles to that of the pen. Care was taken to make it such that a positive or upper position of the crank pin, that is, a positive value of sine Θ, should always produce a positive co-ordinate on the paper or on upper or right-hand position of the pen, when the figure drawn is looked at from above in the normal position of the operator, i. e., facing the flywheel or pen-units side as in Fig. 2. The distinctive characteristic of the machine here described is that it moves the paper in all of its three possible ways, that is, with uniform speed, with simple or compound harmonic motion and in a rotary way. These three ways give rise to as many different classes of curves, but in all three the pen is always moved with the simple or compound harmonic motion already described, while other parts of the machine about to be mentioned are operating only when needed.

I. SINE CURVES

In Fig. 5 the large carriage under the pen carriage is used merely as a table in this class of curves, and is securely tied or locked so as to remain stationary. Not visible in this Fig. 5, and not shown in any of the others, is a roll of paper, 4¼ inches wide, on the motor side of the machine. This paper passes over the stationary carriage under guides, and then down between



Compound sine curves. This gives slightly over one half of the complete curve



Simple and compound sine curves with ratio 8:5. Only one-half of the curves are shown. The remaining portions are the same, but in the reverse order

two equal rollers, only one of which is visible here on account of the paper, while both may be seen somewhat in Fig. 4. One of these rollers carries a gear wheel which meshes into the endless screw of unit A, while the other, the one visible in Fig. 5, is held to the first by two springs. Finally, there is a removable spindle on which the ribbon of paper may be wound by hand.

A PURE SINE CURVE

To begin with the most elementary sine curve, a pure sine curve, we place a 32-cog wheel on one of the units D, E or F on the pen side, adjust its crank pin for a half inch radius so that the pen has an effective amplitude of one inch, and place also a 32-cog wheel on the roller. The gear wheels are all of what is called 32 pitch, that is, a wheel of one inch pitch radius has 32 cogs, a half inch wheel 16, an inch and a half 48, and so on in proportion. The pitch radius is the acting one, so that the centers of two wheels must be placed exactly the sum of their pitch radii apart in order to gear properly. The radii of the rollers are one inch, so that when the machine is set in motion, it will draw a pure sine curve like Fig. 8, whose equation is $y=\sin x$. If we draw the broken straight line AE, the axis of reference or of abscissae, and space on it evenly the arcs of, say, every 10 degrees of a circle with a one-inch radius, erect ordinates or perpendiculars at these points equal to the sines of the respective degrees, and then join their other extremities, we will construct the pure sine curve shown in Fig. 8. This differs from the circle in Fig. 6 in only two particulars, first, in that the sines are evenly spaced; secondly, in that the lower half of the circle is shifted to the right of the upper half. The curve between A and E in Fig. 8 is drawn in one revolution of the crank pin and of the rollers, and represents one cycle. The length AE is the circumference of the circle whose radius is BF and DG, and all the points of the curve are so placed with reference to the line AE that the ordinate y of each is equal to the sine of its abscissae x, that is, of its horizontal distance from A.

SIMPLE SINE CURVES

The pure sine curve of Fig. 8 may be modified in two possible ways, by changing the amplitude BF and the length AE. It will then lose the adjective "pure," but will remain a sine curve, and its equation will be $y=m \sin nx$, in which m and n are coefficients and may have any values whatever. When the amplitude alone is changed, as in Fig. 9, the heights of all the points above and below the reference axis AE may be made larger or smaller in the same ratio, and the curve would then appear steeper or flatter. If the length AE alone is changed, as is done in Fig. 10, the successive waves of the curve would be pulled apart or crowded together just as would happen to a spiral spring that is more or less extended. If both the amplitude and the wave length are made larger or smaller in the same ratio, the curve will remain a pure sine curve on an enlarged or reduced scale. Thus Fig. 11 is the same as Fig. 8 with both its co-ordinates reduced to half the scale. The equations of Figs. 8, 9, 10, 11 when referred to Fig. 8 as a standard are $y=\sin x$, $y=\frac{1}{2}\sin x$, $y=\sin 4x$, $y=\frac{1}{2}\sin 2x$. When referred to Fig. 11 as standard, they are $y=2\sin \frac{1}{2}x$, $y=\sin \frac{1}{2}x$, $y=2\sin 2x$, $y=\sin x$. These and other sine curves are drawn by the machine by changing the radius of the crank pin, the gear wheel on its axle, or the gear wheel on the roller in any combination. An additional gear, not shown in the cuts, may reduce the speed of the rollers two and three times, so that a wide range is obtainable.

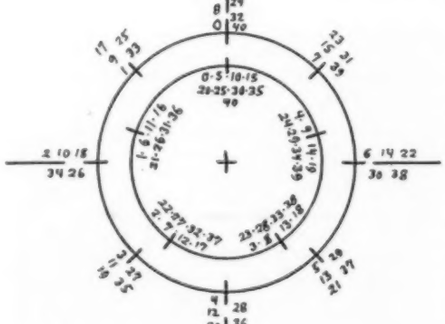
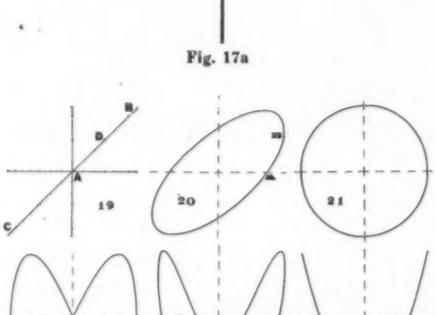
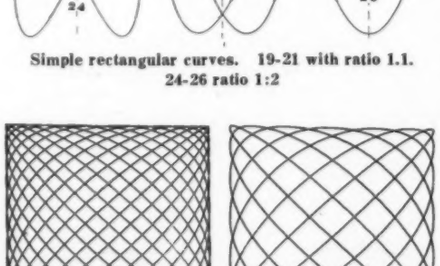


Fig. 17a



Simple rectangular curves. 19-21 with ratio 1.1. 24-26 ratio 1:2



Simple rectangular curves with 22-23 ratio 14-15

COMPOUND SINE CURVES

By using two or more of the units D, E, F, or by adding A, B and C on the other side of the machine, as mentioned before, the sine curves may be compounded and made very complex, each unit having its own period and amplitude and its crank being at any phase angle at an assumed initial position. Fig. 12 shows a combination of the ratios 8 and 9 with equal amplitudes, that is, one curve has 8 cycles while the other has 9. We see how they interfere constructively and destructively at the ends and in the middle of their compound period. This is the phenomenon known as beats in acoustics, and obtains when the notes C and D are sounded together.

Figs. 13 and 14 show the resultants for the components 4, 5, 6, 8, with their maxima and their zero points together respectively at the start, and with equal amplitudes. This is called in music the major chord, and obtains when C, E, G, c are sounded together. The ear cannot distinguish between the two resultants nor between any other phase difference at the start.

Figs. 15 and 16 show two single curves with the ratios of 8 and 5 and unequal amplitudes, like the note c and the E below it. Figs. 17 and 18 give the resultants of the two when they start with their cranks at 90 degrees or at 0 degree together. All four curves, 15, 16, 17, 18, have been marked off to 40ths of their resultant period, so that the reader may try a few points that have the same number on the component curves, add up their ordinates algebraically, and see how the resultant reproduces this sum. To facilitate the process and to prevent confusion, the numbers above the reference axis should be used for one resultant and those below them for the other. We shall make use of these figures again later on. But before leaving them let us note that Fig. 17 is symmetrical with respect to the *perpendicular* at its middle, i. e., at No. 20, and that Fig. 18 is symmetrical with regard to the *point* at its middle, that is, the second half of Fig. 17 will fit the first half perfectly when the paper is folded over around the perpendicular at 20; whereas Fig. 18 must be swung round this point with the same side of the paper facing us. The same would be true if we used the point O instead of 20 in both figures.

The insert, Fig. 17a, may also be used in our study. The inner circle shows the generation of Fig. 15, and the outer one that of Fig. 16. The circumference of the inner one is divided into five parts and that of the outer one into eight, and the corresponding points are numbered. Starting with O on both circles, we add up their ordinates algebraically, that is, their vertical heights above the horizontal diameter, and find their sum equal to the ordinate at O on Fig. 17. We may do the same to each of the successive

points from 1 to 40. By turning the insert at right angles and proceeding in the same way, we may construct Fig. 18. The two circles represent in fact the effective motions of the two crank pins. Curves traced by points in this way are mechanically drawn by the machine, which supplies also all intermediate values.

It will interest young mathematicians to know that Fourier proved that every periodic single-valued function, that is, a curve which repeats itself perfectly in all its details in the same order, no matter how irregular it may look, and which had only one value of y for any one value of x, is a sine curve, and the resultant of two or more components with their own periods, amplitudes and phase angles at any initial moment, so that once these three elements are known the resultant may be drawn. As the curves of the tides, the temperatures, and the like, are single-valued and periodic, they are sine curves. It is due to the acumen of our greatest mathematicians that some of these sine curves have been analyzed into their components. This is the case especially with the tide-predicting machine mentioned before, which can now predict the height of the tide at any place within a couple of inches for almost a century in advance.

II. RECTANGULAR CURVES

Instead of moving the paper with uniform speed, we may give it a harmonic motion. Fig. 3 will explain the process. The paper carriage, which was locked for the sine curves, is now attached to a cord running through one or more of the units, A, B, C, or even D, E, F if desirable. Fig. 3 shows only the unit B in operation. The pulling weight is seen to the right. For the sake of the photograph a white cord was used throughout. In practice a steel wire, No. 27, is used, with a diameter of 0.014 inch.

A SIMPLE RATIO

Let us begin with the simplest case, one unit for the pen such as D, and one for the paper carriage such as B, with gear wheels having the same number of cogs, thus giving a ratio of 1:1. Let the amplitudes also be equal, and let both cranks on the dials D and B show 0 degree. Then the pen will be at A in Fig. 19. As the motor begins to run, the pen will move upwards, say, the sine of 30 degrees, and the paper will move the very same distance to the left, so that the pen will appear to move up and towards the right and be at D in the figure. It reaches the point B when both crank pins read 90 degrees. It will return to A when they read 180 degrees. It will be at C for 270 degrees, and at A again for 360 degrees or 0 degree. That is, the curve will be a straight line inclined equally to the axes. These have been scaled off to the sines of 10 degrees intervals, and so has the line CAB, so that the positions of the pen may be accurately followed.

Now let us change the phase for, say, the paper carriage, making its pointer read 45 degrees on the dial B, while the pen reads 0 degree on dial D. The pen will then be at A in Fig. 20. After one-eighth of a turn, the pen will be at B, dial B reading 90 degrees and carrying the pen farthest to the right, while dial D will show 45 degrees, so that the height of the pen B above the horizontal axis will be the sine of 45 degrees. Following this process step by step with the crank B always 45 degrees ahead of D, we will see how the curve will turn out to be an ellipse, as in Fig. 20. With a phase difference of 90 degrees the ellipse will become a circle as in Fig. 21. Were the amplitudes to be unequal, it would remain an ellipse, but have its greatest width.

If we were to draw a whole series of such ellipses, from the line to the circle, without shifting the paper on its carriage, but merely loosening and retightening one of the gear wheels at intervals of five or ten degrees, and continue the process until the phase difference was 180 degrees, we would find the circle in Fig. 21 changing into ellipses leaning in the opposite direction from that in Fig. 20, until we came to a straight line again at right angles to CB in Fig. 19, when the phase difference would be 180 degrees. A continuation of this phase shifting would carry us back through the same series of ellipses in the reverse order and finally land us again at Fig. 19. As the general class of these curves is an ellipse, we see that the straight line and the circle are really ellipses, the first having its minor axis zero, and the second having it equal to the major axis.

Instead of thus laboriously changing the phase difference so frequently, we may draw the whole series of curves automatically if we take a ratio that is not 1:1 but very near it. Thus Fig. 22 shows the ratio 15:14 with both cranks together at 90 degrees at the upper right hand corner. We see that the pen starts to draw the straight line BC on Fig. 19 from B towards C, but its course soon changes into that of an ellipse, which also is continually varying in eccentricity. The middle ellipse is trying its best, we might say, to develop into a circle, but is changing its inclination in the attempt. There is, therefore, strictly speaking, no true ellipse anywhere in Fig. 22. When the phase difference is slightly altered the right amount, the pen does not retrace its path perfectly as in Fig. 22, but draws the curve shown in Fig. 23. Both 22 and 23 are each, it is hardly necessary to say, one continuous curve.

With the ratio 2:1 in Fig. 24 the pen moves up and down twice while it moves to the right and left only once. With both pointers at 0 degree at the start, the pen is in the middle of the figure. It returns to this position after one revolution of the faster wheel, and again after a second revolution. Fig. 25 shows the same combination with a phase difference at the start of 45 degrees. Fig. 26 gives a true parabola for a difference of 90 degrees. This is traced doubly in the compound cycle.

[TO BE CONTINUED]

Effects of Hearing Versus Seeing

By William J. Dana

It is my conclusion based upon many observations and much study that the mind is comparatively much more sensitive to auditory stimuli than to visual stimuli. In other words what we hear makes more impression upon us than what we see. There are innumerable kinds of pleasant and unpleasant sights and sounds so that it would be impracticable for any one person to enumerate them all but I want to give a few examples to illustrate just what I mean and if possible to prove my contention.

For example we can read about a person being in pain or watch a movie of a person being hurt and be sorry or feel genuinely sympathetic. Or we can even see a person drowning and want to help or do something of assistance but it isn't until we hear the groan of an injured person or the cry for help that we are stirred to the depths of our being to aid instantly.

It seems to me that one of the chief reasons why we here in the United States have been so apathetic over the European war is because we haven't *heard* it yet. As soon as Paris heard the boom of the Big Berthas she awoke to an instant appreciation of the meaning of the German invasion. It wasn't until English coast towns were shelled and London bombarded by Zeppelins that England awoke from her lethargy and realized that there was a war going on in which she was vitally interested. It seems as though we had to be aroused to action through our ears rather than through our eyes. And perhaps the most potent influence is the human voice. This is well illustrated in child training when we see how stubbornly a child will resist a cross or impatient command while the kindly or patient request brings willing compliance. The same thing works out in the shop where men jump to help the cheerful call for aid but are morose and sullen if the boss has a grouch. And how we all hate to be bawled out by our superiors in position! We will do anything rather than get called down for poor work, and on the other hand we want our good work appreciated. It helps tremendously to have the word of commendation occasionally; and kindly criticism in private is also well taken.

A fire in a tall office building or warehouse is a thrilling sight from a distance but as we approach and hear the clanging of the fire engines' bells, the shouts of the firemen, the swish of the streams of water and the calls or screams for assistance we realize that it is a dreadful thing after all.

I have seen men hurt in lots of football games and often seen them carried senseless from the field and have not been much impressed but I shall never forget a certain game in which a brilliant end run was made. As the end was tackled he was turned so that his left leg was broken. The snapping of the bone could be heard all over the stadium and was very startling but the suddenness of the break caused the tackled man to cry out like a hurt rabbit and that cry sickened the whole crowd of spectators and players. It was some time before the players regained sufficient nerve to go on with the game.

There is no doubt that "music hath charms to soothe the savage breast." It also can arouse it very easily. I know of nothing more irritating than some harsh ragtime tune played over and over on a victrola. Other irritating noises are legion in number and the noises of big cities have at last become recognized as nuisances and must be eliminated at least in hospital zones.

The war cry of the American Indians was intended to inspire terror in the hearts of those who heard it and it had that effect. The war dances of the Indians and the wild dances of the negroes in Africa as well as similar religious ceremonial dances in other parts of the world begin with low humming and the light beating of tom-toms and gradually increases in intensity until the participants have worked themselves into a frenzy.

Besides savage breasts there are savage beasts that can be tamed by suitable music. The animal trainers depend much more upon the hearing than upon the vision of their animals. The simplest and most obvious illustration of this is the equestrians whose horses shift from two step to waltz as the circus band changes its time or air. Dogs know at once from the inflection of a stranger's voice whether he is kindly disposed or not.

However, there are a great many pleasant sounds as well as unpleasant ones. Therapeutically it has been often proven that music has very beneficial effects especially during convalescence. Soothing melodies are quite healing in their results. How we love the old familiar airs and hymn tunes we learned as children! The electrical effect of singing "Nearer My God to Thee" during theater fire panics has been proven many times. The sight of a calm man in a turmoil is no doubt reassuring but the sound of a firm confident voice in a crisis is much more effective. The good physician well knows the great value of firm confident positive assertions to his patients.

It is remarkable how the intensity of a sound will affect those hearing it. I remember that during a talk given by a missionary returned from China he tapped very lightly on a large gong. It sounded very beautiful and resonant. Then he tapped louder and louder until he was making a fearful noise and explained that as such it was used by the old style Chinese police to frighten away robbers! I'm sure I would gladly have run away from such a racket and yet when lightly tapped that gong was a wonder.

Of course pleasant surroundings are nice and help toward comfort. We like good pictures in our homes especially those that have a deep perspective such as mountain or marine views but even more restful than these and more relaxing is the quiet happy voice of a loved member of the household.

Perhaps we can never trace just what the evolution of the senses has been but it seems to me as though primitive men must have relied more upon hearing than seeing especially as they were more given to night prowling than we. Now that we have our artificial forms of lighting we use our eyes more and our ears less. But whether the things one hears affects ones actions more than what one sees I am very thankful that I have all my senses in good working order to help me keep in harmony with my environment.

Decolorizing Carbon

Experiments have been carried out by the sectional chemical committee of the Royal Society on the production of decolorizing carbons similar to the German and Dutch products known as "eponite" and "norite." From microscopic examination of various charcoals it was concluded that eponite is made from a mixture of coniferous wood, such as pine or cedar, and angiospermous wood, such as poplar or willow. A preliminary treatment of the wood on the lines of Ostrejko's Eng. Pat. 18,040 of 1900 (this J., 1902, 58) gave improved results. In the experiments wood as sawdust or shavings was boiled with a solution of calcium or magnesium acetate, then dried, and carbonized at a red heat with addition of lime or magnesia, and the charcoal washed with hydrochloric acid, then with water, dried and again heated to redness. The addition of starch mucilage to the treated wood increased the decolorizing power. Charcoals made at higher temperatures (approaching white heat) from a mixture of pine and willow treated with calcium acetate and starch and from cedar wood treated with calcium acetate, were superior to eponite. Experiments were carried out on willow and cedar as two distinct types of wood under the following conditions: (1) Natural wood, without treatment; (2) Wood treated with milk of lime; (3) Wood treated with calcium acetate. These were carbonized at white heat, and the charcoal washed with hydrochloric acid, then with water, and finally heated. Process (1) gave a charcoal useless for decolorizing; (2) and (3) gave very active charcoals, all equal to eponite. The calcium-acetate charcoals were slightly more active han the milk-of-lime material and the type of woodt had little influence on the result. The process used was sawdust from pine, willow, or cedar mixed with milk of lime to form a dough, and the mixture heated to drive off some of the water. The mass was placed in a crucible, covered with a layer of lime, and raised to a temperature approaching white heat. The charcoal was boiled with excess of hydrochloric acid, filtered, washed, drained and heated in an iron retort to a red heat. The charcoal was then powdered and sifted through a No. 60 sieve. For carbonizing a No. 6 salamder crucible heated in a Fletcher radial injection furnace was used.—*Chem. Trade Jour.*

The Geologic Role of Phosphorus*

Study of a Minute Problem in Its Larger Relations

By Eliot Blackwelder

PROGRESS in scientific research is attained largely by the most intensive study of minute problems; but it is also necessary that there should be frequent attempts to view these problems in their larger relations. Such undertakings are always the more hazardous for the student because they oblige him to reach out of his special field into domains with which he may have only a general acquaintance. In preparing this paper on the part played by phosphorus in geologic processes, the writer has tried not only to correlate his work with that of many other students in this and other countries, but also to understand the bearing of sister sciences upon the subject. He has done so with keen realization of the pitfalls that beset the path especially outside his own limited field.[1]

Phosphorus occurs naturally in many different forms and situations. Nevertheless, its varied transformations follow an orderly sequence, which is in harmony with the general scheme of rock metamorphism and biologic evolution. In a broad way, these changes form a cycle within which there are subordinate cycles, all having a common beginning, and probably capable of being brought to a common end. Any particular atom of phosphorus may follow one or more of the subordinate cycles while another atom may pursue a different route.

The primary occurrence of phosphorus in the earth, like the beginnings of most things in geology, lies wholly in the realm of speculation. If the planetesimal theory of the earth's origin,—elaborated by Chamberlin and Moulton,—is true as regards its major points, the original material of the earth may be comparable to the meteorites which are still falling upon its surface from time to time. Nearly all modern meteorites contain minute quantities of phosphorus,—chiefly in the form of the iron-and-nickel phosphide (schreibersite). Individual meteorites vary in the amount of phosphorus which they contain. On the whole, the iron meteorites are generally richer in the phosphide than are the stony varieties, the range being from a trace in the latter to 1.25 per cent., expressed as P_2O_5, in the former.

On the basis of the planetesimal theory, Chamberlin has suggested that more or less of the original cosmic matter of the globe has been melted and worked its way outward to the surface. Meanwhile it should have differentiated until the uppermost part became a familiar volcanic magma. While there are weighty considerations in favor of this concept, it is admittedly speculative. Whether or not it be true, it is a fact that the nearest approach to a primary occurrence of phosphorus actually known to geologists, is its appearance in the igneous rocks, which have crystallized from a hot mineral solution (the "magma" of geologists) rising from the unknown depths of the earth's interior. All igneous rocks contain small quantities of phosphorus. According to Clarke's calculation, the average igneous rock of the world contains .29 per cent. P_2O_5. The ratio is usually somewhat higher (.50-1.15 per cent.) in the more basic igneous rocks such as gabbro and the peridotites. In all these rocks the phosphorus exists chiefly as the mineral apatite,—an anhydrous tricalcium phosphate chemically combined with calcium fluoride or chloride.

When magma crystallizes it emits large quantities of steam and other vapors. At the surface these are discharged into the atmosphere; but around deep-seated intrusions of magma the liquid and volatile constituents permeate the adjacent rocks. The mineral matter with which these solutions are highly charged crystallizes out selectively upon the walls of fissures and other openings through which the hot solutions pass, thus producing among other things the type of veins known as pegmatites. Most pegmatites contain such minerals as quarts, feldspar and mica; but in rare instances, apatite is the chief constituent. Like the other minerals, it usually occurs as large crystals, sometimes a foot or more in length. The so-called nelsonites[2] of Virginia, which contain 5-16 per cent. of P_2O_5, as well as certain vein deposits in Quebec and Norway, are evidently pegmatities. In similar veins some other phosphatic minerals, such as the valuable rare-earth phosphates monazite and xenotime, have been found, but they are decidedly rare.

Certain veins of much less frequent occurrence than the apatites consist largely or even entirely of the related minerals dahllite or staffelite,—considered by the French mineralogist Lacroix[3] to be hydrous calcium carbo-phosphates, containing about 39 per cent. P_2O_5. The mineral occurs in lamellar and radiating aggregates of very minute fibrous crystals, rather than in coarse stout prisms like those of apatite. Although a vein of this type near Crown Point, New York, has been described by Emmons,[4] the only large and well-known examples are those of the province of Estremadura[5] in Spain, where they have been recognized for nearly 150 years. Several different hypotheses have been advanced to account for these Spanish veins, but most of them are clearly inadmissible. The facts that the veins are persistent with depth, contain quartz, and traverse such slightly altered rocks as quartzite, slate and limestone associated with granitic intrusions, suggests that they have crystallized from ascending magmatic solutions but at a moderate temperature. It is not certain, however, that they have not been produced by waters descending from the surface.

Under ordinary climatic conditions, rocks near the surface of the earth are subject to chemical decomposition. Percolating underground water containing more or less carbonic acid and other solvent materials is one of the chief agents in this decay. In such solutions both apatite and dahllite dissolve more readily than most other common minerals, although much less rapidly than the lime-carbonate minerals, such as calcite. The phosphate-bearing solution then circulates through the rock and soils, to be disposed of eventually in several different ways.

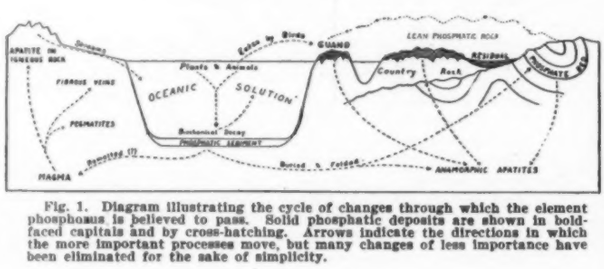


Fig. 1. Diagram illustrating the cycle of changes through which the element phosphorus is believed to pass. Solid phosphatic deposits are shown in bold-faced capitals and by cross-hatching. Arrows indicate the directions in which the more important processes move, but many changes of less importance have been eliminated for the sake of simplicity.

Much of the dissolved phosphoric acid is taken up by plants, whose roots penetrate the soil, and by them is incorporated in the nuclear material of their cells, and particularly in their seeds. Animals taking the phosphorus indirectly from the plants upon which they feed use it not only in various cells and tissues, but also in bones and teeth. In the vast majority of cases part of it is soon returned to the soil as a constituent of urine, fæces and dead organic matter, but bacteria then decompose its compounds and the phosphoric acid returns to the state of solution in ground water. Only rarely, when soon buried in mud, does it become fixed in land deposits in the form of bones and even mineralized fæces (coprolites). On the other hand, thin beds with very limited area, consisting largely of bones and teeth, have been found in a few places, such as Big Bone Lick in Kentucky.

By far the greater part of the phosphorus in ground water solution must either immediately or eventually go into the streams and find its way to the ocean. Of the vast quantity of dissolved mineral matter annually delivered to the sea by the run-off, it is estimated that about .45 per cent. consists of phosphorus pentoxide. Using the best available figures for the amount of water thus brought to the ocean annually, it is calculated that if the phosphatic material in the form of solid tricalcium phosphate were loaded into standard railroad cars it would fill a train stretching continuously from Boston to Seattle and would be 7 to 12 times as great as the world's total production of phosphate rock in 1911. Nevertheless, so great is the volume of the oceans, and so vast the area of their floors, that if all this material were deposited in solid form uniformly over the bottom of the sea, it would build annually a layer less than one-fifth of a millimeter thick. Of the phosphorus poured into the sea, so large a proportion is utilized by living beings that the net working balance dissolved in oceanic water constantly averages less than .005 per cent., expressed as P_2O_5, or, in other words, about .18 per cent. of the dissolved salts. In this solution, phosphorus seems to have reached the most dilute state in which it exists during the course of its complex migrations. Its subsequent transformations, now to be described, generally tend to ever greater concentration, almost until the cycle is closed upon itself.

Soluble phosphates are absorbed by the various oceanic plants as well as by those on land. In some measure the phosphoric acid becomes chemically linked in organic compounds, but for the most part it probably remains in the ionized state. Living diatoms and other algæ contain, in both of these forms, from .1 to .2 per cent. of P_2O_5, chiefly as a minor constituent of the cell nucleus. Although the marine animals have the power of absorbing phosphoric acid directly from the sea water, it so happens that they generally get a surplus of it as a constituent of the plants or other animals on which they feed, and hence do not exercise that power.

The round of transformations to which phosphorus is subjected in the ocean is extraordinarily complex. The plants which absorb the element are devoured by myriads of aquatic animals, each of which, in its turn, is liable to a similar fate. This endless process of devouring is recognized even in the ancient Chinese proverb to the effect that "the big fish eats the little fish, the little fish eats the shrimp, and the shrimp eats the mud." In the individual animal, the phosphorus forms a constituent of its cell nuclei, tissues and liquids. With the exception of casein and that of egg-yolk, the proteins do not contain phosphorus, but it forms a constituent of certain other organic compounds, such as lecithins. As solid calcium phosphate it resides in bones, teeth and more rarely in shells.

As phosphorus ascends in the evolutionary scale of animals, its concentration tends to increase, although irregularly. The protozoan, air dried, contains less than .6 per cent. P_2O_5. According to Juday[6] quantities of minute crustaceans from Lake Mendota contain in the air-dried condition 1.8 to 2.4 per cent. of P_2O_5, or several times that of the protozoans. A Russian biochemist, Sempelovski, found in entire fresh specimens of a cartilaginous fish (the common skate) .91 per cent. P_2O_5, whereas the average for eight Teleostean fishes with well-developed bones was about 1.5 per cent. Certain brachiopods, such as those of the family *Lingulidae*—form shells of fibro-crystalline tricalcium phosphate—probably either the mineral dahllite or staffelite.

From its almost endless series of reincarnations in the ocean, phosphorus is allowed to escape from time to time by either one of two routes. The organisms in the sea may be eaten by land animals, chiefly birds, or the phosphates may become fixed in mineral form in the solid matter on the sea bottom, and eventually buried beneath the accumulating sediments. We may consider the second of these processes first.

As the writer has already remarked, the animals of the sea are almost never permitted to die of old age, but are devoured sooner or later by other animals. Any that happen to die in other ways are almost invariably eaten at once by scavengers. Even the bones of fishes are rapidly devoured by echini and certain other animals. It is conceivable, however, that in rare instances the quantities might be too great for the capacities of the scavenger population; and in that event a local accumulation of animal matter might result. The late Sir John Murray[7] based upon this idea a hypothesis to explain the origin of the phosphatic nodules now dredged up from the sea bottom in several parts of the world. In another work[8] he cited the remarkable case of the tile-fish, which in 1883 were killed by hundreds of millions along the Atlantic coast of the United States, presumably by a sudden fall of water temperature brought about by the shifting of

*Presented in summary before the Geological Society of America at Washington and republished from the *American Journal of Science*.

[1]For valuable information and advice the writer is indebted to his colleagues, Professors E. V. McCollum and E. B. Hart, of the University of Wisconsin, Mr. Chauncey Juday of the Wisconsin Geological and Natural History Survey, and Prof. W. A. Noyes of the University of Illinois.

[2]Watson, T. L., and Taber, S., The Virginia Rutile Deposits, Bull. U. S. Geol. Survey, No. 430, 1910, pp. 202-213.

[3]Lacroix, A., Minéralogie de la France, vol. iv, p. 555 et seq.

[4]Emmons, E., New York State Geological Survey, Report, 1838, p. 252.

[5]Fuchs and de Launay, Traité de Min. et Met., 1893, p. 353.

[6]C. Juday, personal communication.

[7]Murray, Sir John, Challenger Expedition Report, Deep Sea Deposits, pp. 396-399.

[8]Murray, Sir John, Geogr. Journ., vol. xii, p. 113, 1898.

the position of the cold northern current between the Gulf Stream and the coast. Using Murray's figures for the area and number of the fish, the writer estimates that enough were killed at this time to make a layer of fish substance about four millimeters deep over the affected area, if all had fallen to the bottom and had been uniformly distributed. Other instances of this kind have been reported and fish are known to have been killed in great numbers by submarine earthquake shocks, submarine volcanic eruptions, and other catastrophes. It might be supposed that in such cases a layer of bones and teeth would be left upon the bottom of the sea, and if the process were repeated at intervals, the layer might gradually attain noteworthy thickness. It should be remembered, however, that the carcasses of dead vertebrates generally float, because distended by the gases of putrefaction; and both while floating and after lodgment upon the shore they are subject to the attack of scavengers as well as to the final decomposing action of micro-organisms. As a final result, but little of the original fish remains except the points of the teeth, which being almost wholly mineral matter apparently contain too little nutritive substance either to attract the spoilers or to serve the purposes of bacteria. Under these circumstances it is difficult to imagine how a layer of carcasses could be deposited in the open sea. On the assumption, however, that it is possible, Murray outlined a process of fermentive decay and chemical interchange which is essentially that to be detailed below. It is chiefly this source of supply that is here laid open to question.

As an incident in the normal life of vast numbers of organisms, both on the sea floor and in the upper waters, shells and little pellets of excrement are incessantly falling to the sea bottom. Some of the dredgings of the Valdivia expedition[9] showed that over large areas of the sea bottom the latter material forms an appreciable part of the soft ooze, and, in several places the sediments consist almost entirely of such pellets. They have been attributed in large measure to holothurians, echinoids, and marine worms. No chemical analysis of this material is available, but it is well known that animal excreta in general contain a noteworthy proportion of phosphoric acid. Although the phosphorus in the excreta of nearly all animals below the Mammalia seems to exist chiefly in the form of insoluble organic phosphates, bacteria are able to decompose these compounds, usually with the formation of ammonium phosphate which is immediately returned to the oceanic solution and there doubtless exists in the ionized condition. Under ordinary circumstances, as pointed out by Sir John Murray in the Challenger Reports[10], even bones, teeth and shells lying upon the sea bottom gradually lose their phosphoric acid. Hence this fæcal material probably does not accumulate to any considerable depth. In fact, over most of the ocean bottom it is destroyed about as fast as it is produced. In so far as this action prevails, phosphorus cannot well become a solid part of the sediments deposited on the sea floors.

Nevertheless, we find among the rocks derived from oceanic sediments in many parts of the world beds several feet thick which are rich in lime-phosphate and extend rather uniformly over thousands of square miles. They contain marine fossils, which indicate that they have accumulated upon the sea bottom. It is therefore evident that locally there must be conditions which cause the fixation of the phosphoric acid among the bottom sediments. Some students of these deposits have ascribed them to the direct deposition of phosphatic shells, bones and teeth, and others have made appeal to the agency of mineral springs. Generally they have sought an explanation for the abundance of the phosphorus. As the writer has already shown, however, the quantity of phosphorus dissolved in seawater is always sufficient to produce in a few thousand years even the thickest known phosphate beds; and hence we need only to account for the special conditions which cause the fixation of the phosphoric acid There is excellent reason to think that the immediately controlling conditions are chemical or biochemical, but these chemical conditions in turn depend upon physiographic and climatic factors difficult to analyze and estimate. The study of the latter is a task for the geologist.

In its simpler aspects the chemistry of the marine deposition of phosphates has been plausibly interpreted by a number of European students of the question, even as far back as 1870. The following is a modification of their views, based on modern information. The process and results of bacterial decomposition of organic matter vary according to the conditions as well as the particular class of bacteria that are at work. In air and aerated water, decay is generally complete, resulting in the production of carbon dioxide, water, soluble nitrates, sulphates, phosphates, etc. In the absence of oxygen, however, the anerobic bacteria somewhat more slowly break down the organic compounds and produce a different series of end products, of which the most important are various hydrocarbons, nitrogen, ammonia, and hydrogen sulphide, with only so much of the carbonic oxides as the available oxygen in combination permits. In so far as free oxygen is present in only small quantities, there should be a compromise between the two processes.

Some of the most obvious characteristics of our marine phosphatic rocks show that they have been associated in origin with the anerobic phase of bacterial action. Almost invariably they are black in color and, owing to the fact that they contain noteworthy quantities of hydrocarbon oils, tars and gases, they are famous for their bad odor. In central Wyoming[11] phosphate rocks of this kind contain so much oily matter that they are being successfully exploited for petroleum. Although such phosphates contain a few fossils such as fish teeth, brachiopods and larval gastropods, they are invariably devoid of sessile bottom-inhabiting organisms, a fact which suggests that the bottom layer of sea water lacked the oxygen necessary to support life.

The deficiency of oxygen is, therefore, the controlling chemical condition, for it not only determines that the bacterial decay shall be of the anerobic type, but also prevents animal scavengers from devouring such organic matter as may fall to the bottom, for no animal can be active in an oxygen-free medium. Through the work of Birge and Juday[12] on the Wisconsin lakes, and that of other students of lake phenomena, it is now well understood that mechanical circulation of the

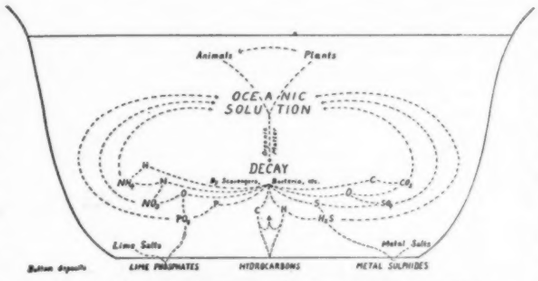


Fig. 2. Diagram to illustrate the process and results of the decay of organic matter on the sea bottom. In reality the process is much too complex to be represented in this way. (The materials which become fixed in the bottom layers are shown in capitals, on the lower line. Other marine sediments, such as articles of sand, mud, sheils and bones, have been omitted from consideration.)

water is the only factor that serves to prevent this deficiency of oxygen from becoming general in all water bodies. At the present time most parts of the ocean bottom are thus supplied with enough oxygen to support their benthic faunas. It is carried in by the slow convective circulation downward from the polar regions and upward near the equator. In order to account for the oceanic phosphate deposits, therefore, we must apparently discover those rare areas of the sea bottom where this circulation is not effective. Deep inclosed gulfs or seas, such as the Black Sea, today afford some of the conditions, but not all of them. The bottom sediment of the Black Sea[13] is now a lifeless mud blackened by hydrocarbons and charged with hydrogen sulphide. There is, however, some condition lacking, for the deposition of phosphates in the Black Sea is not indicated by the dredgings thus far reported.

Passing by this question as a room for which the key is yet to be found, we may consider the manner in which phosphorus comes to be fixed in the oceanic sediments under anerobic conditions wherever they may be developed.

As was long ago pointed out by Bonney[14]—under ordinary circumstances all of the products of decay are likely to either remain in solution or escape as gases rather than to be precipitated. Under special conditions, however, most of them remain in solid form and others react with the sediments of the bottom or with materials in solution, in such a way as to form insoluble products. For example, hydrogen sulphide, interacting with the iron compounds, forms the mineral pyrite, which is common in certain types of black shales. In a similar way, phosphoric acid in the presence of ammonia reacts with various substances, and especially lime carbonates, in such a way as to produce phosphatic minerals, of which the commonest is collophanite,—said to be hydrous calcium carbo-phosphate. These changes have been carried out experimentally in the laboratory by several investigators, and the necessary conditions are such as may readily occur on the sea bottom where organic decomposition is in progress. The calcareous shells and fragments lying on the ocean floor thus become phosphatized, and even such organic materials as excretory pellets and pieces of wood are known to have been altered in the same way. Bones, which initially contained about 58 per cent. tricalcium phosphate, have their organic matter completely replaced by phosphatic minerals, thus raising the ratio to 85 per cent. or more. In addition, collophanite is precipitated in concentric layers around particles of sand or any solids, forming round or elliptical granules which resemble the oölitic grains in certain limestones. By the enlargement of these coatings the granules, shells, teeth and other objects are cemented into hard nodules or even into continuous beds of phosphatic rock. Such nodules have been dredged up from the bottom of all the oceans in moderate depths, and are not uncommon in certain kinds of marine limestones and shales now on land.

The marine phosphatic sediments now constitute our greatest bodies of commercial rock phosphates, exemplified in the phosphate beds of Tunis, Algeria, England, and—most extensive of them all—those of the Rocky Mountains of Idaho, Utah and Wyoming. In many other places, such as the Carolinas, Florida, Belgium, and northern France, marine sediments containing only 1 to 5 per cent. P_2O_5 have, through secondary concentration in later ages, produced rich phosphatic deposits.

Reference has already been made to the fact that, through the agency of land animals such as birds, the phosphorus may escape from the charmed circle of its metamorphoses in the ocean. Upon islands where they are out of reach of predaceous animals seabirds congregate in extraordinary numbers, and the amount of excrement annually deposited by them upon the surface of these islands is large. Ridgeway[15] cites evidence that it accumulates locally at the rate of about 1½ inches per year. The material is comparatively rich in phosphorus, owing in part to the fact that the birds feed largely upon the bony fishes; but it seems to exist chiefly in the form of insoluble organic phosphates which are not affected by unaided rain water. In humid regions, however, bacterial fermenation decomposes these compounds, and the soluble resultants, including phosphoric acid, are removed by rain water so rapidly that no appreciable residue is left.

On those arid islands, however, which are situated under the trade winds and "horse latitudes," neither fermentation nor solution is favored, and hence the guano accumulates from year to year. The well-known deposits on the islets off the coast of Peru, in the Leeward Islands of the Caribbean Sea, and on many of the East Indian islands, serve as examples. As compared with its ratio in the fishes and other marine animals, the concentration of the phosphorus in the freshly deposited excrement of the fish-eating birds is about the same,—averaging but little more than one per cent. P_2O_5. In the thoroughly dry guano of the desert islands off the Peruvian coast, where almost no chemical change has taken place, it contains 10-16 per cent. P_2O_5, as well as a noteworthy quantity of nitrogenous and other organic compounds.

On the other hand, where underground water has access to the older portion of the deposit, the guano is more or less fermented, probably by such micro-organisms as the bacteria, with the result that the nitrogenous matter is largely converted into nitrates and ammonia, while the phosphorus forms calcium, magnesium and ammonium phosphates. The occasional rains dissolve out the more soluble ammonium phosphates and nearly all the nitrates, leaving the relatively insoluble alkaline earth phosphates to form a residue of solid "stone guano." The latter contains from 28 to as much as 39 per cent. P_2O_5, largely in the form of hydrous acidic and basic calcium phosphates, closely, and probably chemically, associated with more or less lime-carbonate. The commercially exploited guanos on Baker island in the tropical Pacific, and many others, appear to have passed through this type of alteration.

The strongly phosphatic solutions thus derived from the guano sink downward through the underlying rocks and produce characteristic alterations in them. Where the rock is limestone, it is somewhat rapidly converted into a mass of calcium phosphates, in which the min-

[9] Murray, Sir John, and Philippi, E., Wissensch. Ergebn. der deutschen Tiefsee Exped., Bd. x, Lf. 4, 1905, p. 103. (Carl Chun, editor.)
[10] Loc. cit.
[11] Woodruff, E. G., The Lander Oil Field, Fremont County, Wyoming, Bull. U. S. Geol. Survey, No. 452, 1911.
[12] Birge, E. A., and Juday, C., The Inland Lakes of Wisconsin, Bull. No. 22, Wis. Geol. & Nat. Hist. Survey, 1911.
[13] Andrussov, N., La Mer Noire, Guide des Excursions der 7me Congrès Géologique International, No. 29.
[14] Bonney, T. G., Cambridgeshire Geology.
[15] Quoted by G. P. Merrill, Non-metallic Minerals, p. 267.

eral species are various, although collophanite seems to predominate. On Christmas Island, in the western part of the Pacific Ocean, Willis[10] found that coral limestone had been changed to calcium phosphate to a depth of from 2 to 3 feet within 20 years. In the laboratory Collet[11] immersed a coral skeleton in a weak solution of ammonium phosphate, with the result that the coral was 60 per cent. phosphatized in only two months. Still more remarkable cases have been reported in which refractory volcanic rocks such as andesite and trachyte have been phosphatized by solutions descending from guano beds.

For many years it was generally supposed that the rich and important phosphate deposits of Florida and the Carolina coast had been produced by solutions from guano beds percolating down into limestones, and thus changing them into calcium phosphate; but now it is fairly well established that the phosphatic solutions were derived not from guano, but from marine clays, containing 1-5 per cent. P_2O_5. It has been demonstrated that these clay beds originally overlie the limestone, but have been rather generally stripped off by erosion in more recent time. Although poor in phosphates the clays are hundreds of feet thick and have, therefore, yielded a vast amount of phosphoric acid. In other countries, there are many illustrations of the phosphatized limestone type, and in most cases, as in Florida, the source of the phosphorus was not guano, but a lean phosphatic clay or chalk. The plateau of southern France, the Lasne valley in Germany, and southwestern Belgium furnish well-known examples.

Where the slightly phosphatic original rock was chalk or limestone, a variation of the process has been brought about because the lime carbonate is relatively more soluble than the lime phosphate. Therefore, during the slow process of solution by rainwater descending from the surface, the calcium phosphate, although actually decreased in total quantity, has been relatively concentrated by differential solution. Cases of this kind have been reported from southern England, northern France, and Belgium.

Phosphate beds of both of these secondary types are irregular in thickness, and rest upon a most uneven and cavernous surface of the corroded limestone beneath. The deposits often contain phosphatized bones and shells of animals really belonging to a geologic age much more recent than the limestone of which they appear to form a part. These have slumped in from the surface and been thus mixed with what is in reality onyl a special type of residual soil.

Although the concentration of phosphorus thus brought about by the phosphatization and differential solution of limestones is never quite equal to that in the leached guanos, it may rise to over 36 per cent. P_2O_5, a ratio which indicates almost pure collophanite. Sellards[12] finds reason to think that the amorphous and probably colloidal mineral collophanite is gradually converted into a fibrous crystalline mineral (staffelite?) which has about the same composition. If this opinion is correct, it affords another illustration of the well-known tendency of colloidal or amorphous minerals to assume the crystalline state with the lapse of time.

With certain minor exceptions the transformations of phosphorus that take place on the surface of the earth have now been reviewed. It remains to trace the element downward into the interior of the crust and at the same time more deeply into the realm of inference. Phosphatic deposits of any of the types already described may have sediments deposited upon them until, in the course of geologic ages, they may be buried thousands of feet below the surface. In that region pressures are great, temperatures are much increased, and the activity of solutions is greater than, or at least different from, that above. In the past, some of the older phosphate beds have in addition been subjected to overwhelming compressive forces, the origin of which is still a debatable subject with geologists, although we see plenty of evidence of their operation in the folded rocks of our mountain systems. It is now well known that under these conditions of the interior of the earth, minerals of various kinds undergo radical changes. Some, like calcite, merely recrystallize in more compact form. Others recombine to form new minerals, while still others are metamorphosed by either losing or gaining constituents. Although no case of this kind for phosphates has yet been proven, it is entirely in harmony with the established principles of rock metamorphism for us to suppose that the phosphatic sediments would, under these conditions, be reorganized. The hydrous minerals and the carbonates characteristic of the surface should become dehydrated and decarbonated. As a result, collophanite and staffelite as well as many other minerals of less importance, should pass over into the anhydrous calcium fluophosphate, apatite, in which the proportion of P_2O_5 may rise to 42-43 per cent.—which is apparently the maximum concentration attainable in rocks. It was suggested many years ago by Sir William Dawson[13] and others that some of the rich apatitic beds that are intimately associated with the ancient Grenville marbles and gneisses near Ottawa, Canada, are really the highly metamorphosed representatives of plhosphatic sediments which were once deposited on the bottom of the sea. No weighty arguments against their hypothesis have been advanced.

There is still another fate that may befall the phosphatic deposits either before or after recrystallization. Rocks of any kind in the crust are liable sooner or later to be invaded by large bodies of fluid magma, issuing from the interior of the earth, at initial temperatures well above a thousand degrees centigrade. It now seems proven beyond dispute that such igneous bodies flux their way through the overlying rocks, absorbing them or dissolving them as they rise. Phosphatic minerals thus dissolved would later recrystallize out along with the other constituents of the magma when it became a solid igneous rock. It would then also appear as the mineral apatite, which is apparently one of the few phosphates adapted to these conditions of high temperature. As the constituents of the magma obey the laws of solutions and are diffused uniformly through the liquid, the apatite, like the feldspars, quartz, and mica, would be evenly distributed throughout the resulting igneous rock in the form of minute crystals. In so far as this occurrence takes place, it closes the cycle, for it will be remembered that almost at the outset of this review, phosphorus appeared in the form of microscopic apatite prisms as a constituent of the typical igneous rock. Meanwhile, however, the phosphorus may have passed through the complex series of migrations and transformations over and over again, on and near the surface of the earth. Even if the cycle should thus become closed, it would be closed only temporarily, for a new cycle would be initiated just as soon as the apatite of the second generation became subject to the process of weathering.

[10] Willis, J. L., Ottawa Naturalist, vol. vi, p. 18, 1892.
[11] Collet, L. W., Proc. Royal Soc. Edinburgh, vol. xxv, p. 882.
[12] Sellards, E. H., 5th Ann. Rep. Florida State Geol. Survey, 1913, pp. 37-66.
[13] Sir J. W. Dawson, Note on the Phosphates of the Laurentian and Cambrian Rocks of Canada, Quart. Journ. Geol. Soc. of London, vol. xxxii, pp. 285-291, 1876.

The Complexity of the Chemical Elements—II*

Electrical Relations, Radio-Active and the Nuclear Theory

By Prof. Frederick Soddy, F. R. S.

Concluded from Scientific American Supplement No. 2196, Page 80, February 2, 1918

Practically simultaneously with the first announcement of these results for thorium lead, a series of investigations was published on the atomic weight of lead from uranium minerals by T. W. Richards and collaborators at Harvard, Maurice Curie in Paris, and Hönigschmid and collaborators in Vienna, which show that the atomic weight is lower than that of ordinary lead. The lowest result hitherto obtained is 206.046, by Hönigschmid and Mlle. Horovitz, for the lead from the very pure crystallized pitch-blende from Morogoro (German East Africa), whilst Richards and Wadsworth obtained 206.085 for a carefully selected specimen of Norwegian cleveite. Numerous other results have been obtained, as, for example, 206.405 for lead from Joachimsthal pitch-blende, 206.82 for lead from Ceylon thorianite, 207.08 for lead from monazite, the two latter being mixed uranium and thorium minerals. But the essential proportion between the two elements has not, unfortunately, been determined. Richards and Wadsworth have also examined the density of their uranium lead, and in every case they have been able to confirm the conclusion that the atomic volume of isotopes is constant, the uranium lead being as much lighter as its atomic weight is smaller than common lead. Many careful investigations of the spectra of these varieties of lead show that the spectrum is absolutely the same so far as can be seen.

THORIUM AND IONIUM

A second quite independent case of a difference in atomic weight between isotopes has been established. It concerns the isotopes thorium and ionium, and it is connected in an important way with the researches of which, on two previous occasions, I have given an account here, the researches on the growth of radium from uranium which have been in progress now for fourteen years. It is the intervention of ionium with its very long period of life which has made the experimental proof of the production of radium from uranium such a long piece of work. Previously only negative results were available. One could only say from the smallness of the expected growth of radium that the period of average life of ionium must be at least 100,000 years, forty times longer than that of radium, and, therefore, that there must be at least forty times as much ionium by weight as radium in uranium minerals, or at least 13.6 grams per 1,000 kilos of uranium. Since then further measurements carried out by Miss Hitchins last year have shown definitely for the first time a clear growth of radium from uranium in the largest preparation, containing 3 kilos of uranium, and this growth, as theory requires, is proceeding according to the square of the time. In three years it amounted to 2×10^{-11} grams of radium, and in six years to just four times this quantity. From this result it was concluded that the previous estimate of 100,000 years for the period of ionium, though still of the nature of a minimum rather than a maximum, was very near to the actual period.

Joachimsthal pitch-blende, the Austrian source of radium, contains only an infinitesimal proportion of thorium. An ionium preparation, separated by Auer von Welsbach from 30 tons of this mineral, since no thorium was added during the process, was an extremely concentrated ionium preparation. The atomic weight of ionium—calculated by adding to the atomic weight of its product, radium, four for the α particle expelled in the change—is 230, whereas that of thorium, its isotope, is slightly above 232. The question was whether the ionium-thorium preparation would contain enough ionium to show the difference. Hönigschmid and Mlle. Horovitz have made a special examination of the point, first re-determining as accurately as possible the atomic weight of thorium, and then that of the thorium-ionium preparation from pitch-blende. They found 232.12 for the atomic weight of thorium, and by the same method 231.51 for that of the ionium-thorium. A very careful and complete examination of the spectra of the two materials showed for both absolutely the same spectrum and a complete absence of impurities.

If the atomic weight of ionium is 230, the ionium-thorium preparation must, from its atomic weight, contain 30 per cent. of ionium and 70 per cent. of thorium by weight. Prof. Meyer has made a comparison of the number of α particles given per second by this preparation with that given by pure radium, and found it to be in the ratio of 1 to 200. If 30 per cent. is ionium, the activity of pure ionium would be one-sixtieth of that of pure radium, and its period some sixty times greater, or 150,000 years. This confirms in a very satisfactory manner our direct estimate of 100,000 years as a minimum, and incidentally raises rather an interesting question.

My direct estimate involves directly the period of uranium itself, and if the value accepted for this is too high, that for the ionium will be correspondingly too low. Now, on May 11 Prof. Joly was bringing before you, I believe, some of his exceedingly interesting work on pleochroic halos from which he has grounds for the conclusion that the accepted period of uranium may be too long.

But since I obtained, for the period of ionium, a minimum value two-thirds of that estimated by Meyer

*Discourse delivered at the Royal Institution and reported in *Nature*.

from the atomic weight, it is difficult to believe that the accepted period of uranium can have been overestimated by more than 50 per cent. of the real period. The matter could be pushed to a further conclusion if it were found possible to estimate the percentage of thorium in the thorium-ionium preparation, a piece of work that ought not to be beyond the resources of radio-chemical analysis. This would then constitute a check on the period of uranium as well as on that of ionium. Such a direct check would be of considerable importance in the determination of geological ages.

The period of ionium enables us to calculate the ratio between the weights of ionium and uranium in pitchblende as 17.4 to 10^6, and the doctrine of the non-separability of isotopes leads directly to the ratio between the thorium and uranium in the mineral as 41.7 to 10^6. This quantity of thorium is unfortunately too small for direct estimation. Otherwise it would be possible to devise a very strict test of the degree of non-separability. As it is, the work is sufficiently convincing. Thirty tons of a mineral containing a majority of the known elements in detectable amount, in the hands of one whose researches in the most difficult field of chemical separation are world-renowned, yield a preparation of the order of one-millionth of the weight of the mineral, which cannot be distinguished from pure thorium in its chemical character. Anyone could tell in the dark that it was not pure thorium, for its α activity is 30,000 times greater than that of thorium. This is then submitted to that particular series of purifications designed to give the purest possible thorium for an atomic weight determination, and it emerges without any separation of the ionium, but with a spectrum identical with that of a control specimen of thorium similarly purified. The complete absence of impurities in the spectrum shows that the chemical work has been very effectively done, and the atomic weight shows that it must contain 30 per cent. by weight of the isotope ionium, a result which agrees with its α activity and the now known period of the latter.

DETERMINATION OF ATOMIC WEIGHTS

The results enumerated thus prove that the atomic weight can no longer be regarded as a natural constant, or the chemically pure element as a homogeneous type of matter. The latter may be, and doubtless often is, a mixture of isotopes varying in atomic weight over a small number of units, and the former then has no exact physical significance, being a mean value in which the proportions of the mixture as well as the separate atomic weights are both unknown. New ideals emerge and old ones are resuscitated by this development. There may be, after all, a very simple numerical relation between the true atomic weights. The view that seems most probably true at present is that while hydrogen and helium may be the ultimate constituents of matter in the Proutian sense, and the atomic weights therefore approximate multiples of that of hydrogen, small deviations, such as exist between the atomic weights of these two constituent elements themselves, may be due to the manner in which the atom is constituted, in accordance with the principle of mutual electromagnetic mass, developed by Silberstein and others. The electro-magnetic mass of two charges in juxtaposition would not be the exact sum of the masses when the charges are separated. The atomic weight of hydrogen is 1.0078 in terms of that of helium as 3.99, and that the latter is not exactly four times the former may be the expression of this effect. Harkins and Wilson have recently gone into the question with some thoroughness, and the conclusion of most interest in the present connection which appears to emerge is in favor of regarding most of the effect to occur in the formation of helium from hydrogen, and very little in subsequent aggregations of the helium. In the region of the radio-elements, where we have abundant examples of the expulsion of helium atoms as α particles, it seems as if we could almost safely neglect this effect altogether. Thus radium has the atomic weight almost exactly 226, and the ultimate product almost exactly 206, showing that in five α- and four β-ray changes the mean effect is nil, and the atomic weights are, moreover, integers in terms of oxygen as 16, or helium 4. It is true that the atomic weights of both thorium and uranium are between 0.1 and 0.2 greater than exact integers, but it is difficult to be sure that this difference is real.

When, among the light elements, we come across a clear case of large departure from an integral value, such as magnesium, 24.32, and chlorine, 35.46, we may reasonably suspect the elements to be a mixture of isotopes. If this is true for chlorine, it suggests a most undesirable feature in the modern practice of determining atomic weights. More and more the one method has come to be relied upon, the preparation of the chloride of the element and the comparison of its weight with that of the silver necessary to combine with the chlorine, and with the weight of the silver chloride formed.

Almost the only practical method, and that a very laborious and imperfect one, which may be expected to resolve a mixture of isotopes is by long-continued fractional gaseous diffusion, which is likely to be the more effective the lower the atomic weight. Assume, for example, that chlorine were a mixture of isotopes of separate atomic weights 34 and 36, or 35 and 36. The 34 isotope would diffuse some 3 per cent. faster than the 36, and the 35 some 1.5 per cent. faster.

The determination of the atomic weight of chlorine in terms of that of silver has reached now such a pitch of refinement that it should be able to detect a difference in the end fractions of the atomic weight of chlorine, if chlorine or hydrogen chloride were systematically subjected to diffusion. It is extremely desirable that such a test of the homogeneity of this gas should be made in this way.

Clearly a change must come in this class of work. It is not of much use starting with stuff out of a bottle labelled "purissimum," or "garantirt," and determining to the highest possible degree of accuracy the atomic weight of an element of unknown origin. The great pioneers in the subject, like Berzelius, were masters of the whole domain of inorganic chemistry, and knew the sources of the elements in Nature first-hand. Their successors must revert to their practice and go direct to Nature for their materials, must select them carefully with due regard to what geology teaches as to their age and history, and, before carrying out a single determination, they must analyze their actual raw materials completely, and know exactly what it is they are dealing with. Much of the work on the atomic weight of lead from mixed minerals is useless from failure to do this. Workers must rely more on the agreement, or disagreement, of a great variety of results by methods and for materials as different as possible than on the result of a single method pushed to the limit of refinement for an element provisionally purified by a dealer from quite unknown materials. The preconceived notion that the results must necessarily agree if the work is well done must be replaced by a system of co-operation between the workers of the world checking each other's results for the same material. A year ago anyone bold enough to publish atomic weight determinations which were not up to the modern standards of agreement among themselves would have been regarded as having mistaken his vocation. If these wider ideals are pursued, all the labor that has been lavished in this field, and which now seems to have been so largely wasted, may possibly bear fruit, and where the newer methods fail, far beyond the narrow belt of elements which it is possible to watch changing, the atomic weight worker may be able to pick up the threads of the great story. No doubt it is writ in full in the natural records preserved by rock and mineral, and the evidence of the atomic weights may be able to carry to a triumphant conclusion the course of elementary evolution, of which as yet only an isolated chapter has been deciphered.

THE STRUCTURE OF THE ATOM

The third line of recent advance which does much to explain the meaning of isotopes and the periodic law starts from Sir Ernest Rutherford's nuclear theory of the atom, which is an attempt to determine the nature of atomic structure, which again is the necessary preliminary to the understanding of the third aspect in which the elements are, or may be, complex. That uranium and thorium are built up of different isotopes of lead, helium and electrons is now an experimental fact, since they have been proved to change into these constituents. But the questions how they are built up, and what is the nature of the non-radio-active elements, which do not undergo changes, remain unsolved.

Prof. Bragg showed in 1905 that the α particles can traverse the atoms of matter in their path almost as though they were not there. So far as he could tell—and the statement is still true of the vast majority of α particles colliding with the atoms of matter—the α particle ploughs its way straight through, pursuing a practically rectilinear course, losing slightly in kinetic energy at each encounter with an atom until its velocity is reduced to the point at which it can no longer be detected. From that time the α particle became, as it were, a messenger that could penetrate the atom, traverse regions which hitherto had been bolted and barred from human curiosity, and on re-emerging could be questioned, as it was questioned effectively by Rutherford, with regard to what was inside. Sir J. J. Thomson, using the electron as the messenger, had obtained valuable information as to the number of electrons in the atom, but the massive material α particle alone can disclose the material atom. It was found that though the vast majority of α particles re-emerge from their encounters with the atoms practically in the same direction as they started, suffering only slight hither and thither scattering due to their collisions with the electrons in the atom, a minute proportion of them suffer very large and abrupt changes of direction. Some are swung around, emerging in the opposite to their original direction. The vast majority, that get through all but undeflected, have met nothing in their passage save electrons, 8,000 times lighter than themselves. The few that are violently swung out of their course must have been in collision with an exceedingly massive nucleus in the atom, occupying only an insignificant fraction of its total volume. The atomic volume is the total volume swept out by systems of electrons in orbits of revolution round the nucleus, and beyond these rings or shells guarding the nucleus it is ordinarily impossible to penetrate. The nucleus is regarded by Rutherford as carrying a single concentrated positive charge, equal and opposite to that of the sum of the electrons.

Chemical phenomena deal almost certainly with the outermost system of detachable or valency electrons alone, the loss or gain of which conditions chemical combining power. Light spectra originate probably in the same region, though possibly more systems of electrons than the outermost may contribute, while the X-rays and γ rays seem to take their rise in a deep-seated ring or shell around the nucleus. But mass phenomena, all but an insignificant fraction, originate in the nucleus.

In the original electrical theory of matter the whole mass of the atom was attributed to electrons, of which there would have been required nearly 2,000 times the atomic weight in terms of hydrogen as unity. With the more definite determination of this number and the realization that there were only about half as many as the number representing the atomic weight, it was clear that all but an insignificant fraction of the mass of the atom was accounted for. In the nuclear hypothesis this mass is concentrated in the exceedingly minute nucleus. The electromagnetic theory of inertia accounts for the greater mass if the positive charges that make up the nucleus are very much more concentrated than the negative charges which constitute the separate electrons. The experiments on scattering clearly indicated the existence of such a concentrated central positive charge, or nucleus.

The mathematical consideration of the results of α-ray scattering, obtained for a large number of different elements, and for different velocities of α rays, further evidence that the number of electrons, and therefore the + charge on the nucleus, is almost exactly the number representing the atomic weight.

Van der Broek, reviving an isolated suggestion from a former paper full of suggestions on the periodic law, which were, I think, in every other respect at fault, pointed out that closer agreement with the theory would be obtained if the number of electrons in the atom, or the nuclear charge, was the number of the place the element occupied in the periodic table. This is now called the atomic number, that of hydrogen being taken as 1, helium 2, lithium 3, and so on to the end of the table, uranium 92, as we now know. For the light elements it is practically half the atomic weight, for the heavy elements rather less than half.

I pointed out that this accorded well with the law of radio-active change that had been established to hold over the last thirteen places in the periodic table. This law might be expressed as follows:—The expulsion of the α particle carrying two positive charges lowers the atomic number by two, while the expulsion of the β particle, carrying a single negative charge, increases it by one. In ignorance of van der Broek's original suggestion, I had, in representing the generalization, shown the last thirteen places as differing unit by unit in the number of electrons in the atom.

Then followed Moseley's all-embracing advance, showing how from the wave-lengths of the X-rays, characteristic of the elements, this conception explained the whole periodic table. The square roots of the frequency of the characteristic X-rays are proportional to the atomic numbers. The total number of elements existing between uranium and hydrogen could thus be determined, and it was found to be ninety-two, only five of the places being vacant. The "exceptions" to the periodic law, such as argon and potassium, nickel and cobalt, tellurium and iodine, in which an element with higher atomic weight precedes instead of succeeding one with lower, were confirmed by the determination of the

atomic numbers in every case. From now on, this number, which represents the + charge on the nucleus rather than the atomic weight, becomes the natural constant which determines chemical character, light and X-ray spectra, and, in fact, all the properties of matter except those that depend directly on the nucleus—mass and weight on one hand, and radio-active properties on the other.

What, then, were the isotopes on this scheme? Obviously they were elements with the same atomic number, the same *net* charge on the nucleus, but with a differently constituted nucleus. Take the very ordinary sequence in the disintegration series, one α and two β rays being successively expelled in any order. Two + and two — charges have been expelled, the *net* charge of the nucleus remains the same, the chemical character and spectrum the same as those of the first parent, but the mass is reduced four units because a helium atom, or rather nucleus, has been expelled as an α particle. The mass depends on the *gross* number of + charges in the nucleus, chemical properties on the difference between the gross numbers of + and — charges. But the radio-active properties depend not only on the gross number of charges, but on the constitution of the nucleus. We can have isotopes with identity of atomic weight, as well as of chemical character, which are different in their stability and mode of breaking up. Hence we can infer that this finer degree of isotopy may also exist among the stable elements, in which case it would be completely beyond our present means to detect. But when transmutation becomes possible such a difference would be at once revealed.

The case is not one entirely of academic interest, because it is probable that the reconciliation of the conflicting views of the geologists and chemists who concluded that lead was not the ultimate product of thorium, and those who by atomic weight determinations on the lead have shown that it is, depends probably on this point.

As has long been known, thorium-C, an isotope of bismuth, disintegrates dually. For 35 per cent. of the atoms disintegrating, an α ray is expelled, followed by a β ray. For the remaining 65 per cent. the β ray is first expelled, and is followed by the α ray. The two products are both isotopes of lead, and both have the same atomic weight, but they are not the same. More energy is expelled in the changes of the 65 per cent. fraction than in those of the 35 per cent. Unless they are both completely stable a difference of period of change is to be anticipated.

The same thing is true for radium-C, though here all but a very minute proportion of the atoms disintegrating follow the mode followed by the 65 per cent. in the case of thorium-C. The product in this case, radium-D, which, of course, is also an isotope of lead, with atomic weight 210, is *not* permanently stable, though it has a fairly long period, twenty-four years. The other product is not known to change further, but then, even if it did, it is in such small quantity that it is doubtful whether the change would have been detected. But, so far as is known, it forms a stable isotope of lead of atomic weight 210, formed in the proportion of only 0.03 per cent. of the whole.

Now the atomic weight evidence merely shows that *one* of the two isotopes of lead formed from thorium is stable enough to accumulate over geological epochs, and it does not necessarily follow that both are. Dr. Arthur Holmes has pointed out to me that the analysis I gave of the Ceylon thorite leads to a curiously anomalous value for the age of the mineral. The quantity of thorium lead per gram of thorium is 0.0062, and this, divided by the rate at which the lead is being produced, 4.72×10^{-11} grams of lead per gram of thorium per year, gives the age as 131 million years. But a Ceylon pitch-blende, with uranium 72.88 per cent., and lead 4.65 per cent., and ratio of lead to uranium as 0.064, gives the age as 512 million years. Dr. Holmes regards the two minerals as likely to be of the same age, and the pitch-blende to be, of all the Ceylon results, the one most truthworthy for age measurement.

If we suppose that, as in the case of radium-D, the 65 per cent. isotope of lead derived from thorium is *not* stable, and that only the 35 per cent. isotope accumulates, the age of the mineral would be 375 million years, which the geologists are likely to consider much nearer the truth. But the most interesting point is that, if we take the atomic weight of the lead isotope derived from uranium as 206.0, and that derived from thorium as 208.0, and calculate the atomic weight of the lead in Ceylon thorite, assuming it to consist entirely of uranium lead and of only the 35 per cent. isotope from thorium, we get the value 207.74, which is exactly what I found from the density, and what Prof. Hönigschmid determined (207.77) (compare *Nature*, May 24, p. 244).

The question remains: If this is what occurs, what does this unstable lead change into? If an α particle were expelled mercury would result, or if a β particle bismuth, two elements of which I could find no trace in the lead group separated from the whole 20 kilos of mineral. But if an α and a β particle were both expelled, the product would be thallium, which is present in amount small, but sufficient for chemical as well as spectroscopic characterization. If the process of disintegration does proceed as suggested, it should be possible to trace it, for this particular lead should give a feeble specific α or β radiation, in addition, of course, to that due to other lead isotopes. So far it has not been possible to test this. In the meantime, the explanation offered is put forward provisionally as being consistent with all the known evidence.

Looking for a moment, in conclusion, at the broader aspects of the new ideas of atomic structure, it seems that though a sound basis for further development has been roughed out, almost all the detail remains to be supplied. We have got to know the nucleus, but, beyond the fact that it is constituted, in heavy atoms, of nuclei of helium and electrons, nothing is known; whilst, as regards the separate shells or rings of electrons which neutralize its charge and are supposed to surround it like the shells of an onion, we really know nothing yet at all. The original explanation, in terms of the electron, of the periodicity of properties displayed by the elements still remains all that has been attempted. We may suppose that as we pass through the successive elements in the table one more electron is added to the outermost ring for each unit increase in the charge on the nucleus, or atomic number, and that when a certain number, 8 in the early part of the table, and 18 in the later, has been added, a complete new shell or ring forms, which no longer participates directly in the chemical activities of the atom. Thanks, however, to Moseley's work, this, now, is not sufficiently precise. For we know the exact number of the elements and the various atomic numbers at which the remarkable changes, in the nature of the periodicity displayed, occur. Any real knowledge in this field will account not only for the two short initial periods, but also for the curious double periodicity later on, in which the abrupt changes of properties in the neighborhood of the zero family alternate with the gradual changes in the neighborhood of the eighth groups. The extraordinary exception to the principle of the whole scheme presented by the rare-earth elements remains a complete enigma, none the less impressive because, beyond them in the table, the normal course is again resumed and continues to the end. This latter, highly significant, feature of the periodic table is one of the definite conclusions following from the chemical characterizations of the numerous radio-elements.

After-Ripening of Sugar Cane

In the Gurdaspur District, India, the stripped cane is allowed to remain in heaps for a day or so before crushing, it being thought by the peasant farmers that the *gur* obtained is lighter in color and drier than that resulting from cane crushed immediately. Results obtained from numerous experiments with Katha or Chin and other local varieties showed that storing the cane in this way tends to further ripening with increase of the sucrose content; but that, on the other hand, there is a danger of loss of sugar by inversion and decomposition if more than a certain time, depending upon the temperature of the air and the condition of the cane, be exceeded. The system used in Java of covering cut cane with damp "trash" in the event of the breakdown of the milling plant, is applicable only so long as the cell protoplasm is alive and the ordinary chemical processes of the cell can continue. When the cane is dead, chemical reactions of hydrolysis and oxidation by enzymes get beyond control, and the rate of sugar decomposition becomes very high.—Note in *Jour. Soc. Chem. Ind.*, on an article by J. H. Barnes in *Agric. J. India.*

How Long Would It Take a Stone to Fall to the Center of the Earth?

This problem exercised the minds of several geometers of the past century, and they arrived at widely differing results. Mersenne stated that, if a shaft could be driven down to the center of the earth, it would take a stone six hours to fall from the earth's surface to the center, while Gassendi only wanted 20 minutes for the same fall. According to the recent calculations of Maurice Sauger (*Comptes Rendus*, vol. 164, pages 954-957), Gassendi was right. As the stone approaches the center, it is drawn downward by the core of the earth, and upward by the shell which it has already penetrated. The rate at which the density of the earth varies or increases as we penetrate to greater depth is unknown. Assuming an average density of 5.53, the time of fall should be 1,234 seconds, Sauger finds; that would be 20 minutes 34 seconds. If the density S should vary according to the relation $p = 10 (1 - 0.76\, r^3/R^3)$, where r is the distance of the spot from the center of the earth, and R its distance from the surface, the time of the fall would come out 19 minutes 15 seconds. The two values differ only by 79 seconds, a little more than a minute. Sauger's formula is based upon considerations of the moment of inertia of the earth, as calculated from the precession of the equinoxes, which agree with observations on the density of the earth conducted in mine shafts. If the shaft were driven right through the earth, the stone would appear at the antipodes after 38 minutes and 30 seconds, and return to its starting point, at which it would make its reappearance at the end of 1 hour 17 minutes.

SCIENTIFIC AMERICAN SUPPLEMENT

Founded 1876

NEW YORK, SATURDAY, FEBRUARY 9, 1918

Published weekly by Munn & Company, Incorporated
Charles Allen Munn, President; Frederick Converse Beach, Secretary; Orson D. Munn, Treasurer
all at 233 Broadway, New York

Entered at Post Office of New York, N. Y., as Second Class Matter

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The Scientific American Publications

Scientific American Supplement (established 1876) per year $5.00
Scientific American (established 1845) 4.00
The combined subscription rates and rates to foreign countries, including Canada, will be furnished upon application.
Remit by postal or express money order, bank draft or check.

Munn & Co., Inc., 233 Broadway, New York

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